Science Exits the White House

science and public affairs

BULLETIN OF THE ATOMIC SCIENTISTS

Continuing our series " The Fight for Peace and You " . . . . . .

Embosograf Company
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Dear Sirs:

I have been following your series of advertisements on ' what can one man do to contribute toward a better world?'

I notice that the problem of disarmament has not appeared in your series. I realize that to many people the danger of nuclear war no longer seems imminent as it has in the past.

But, despite some noteworthy advances this danger is still with us, presenting just as frightening a vision of the apocalypse as it ever has.

I urge you and your readers not to lose their vigilance, to continue to work for disarmament and towards a decrease in the power of the military. The publics' awareness of these dangers needs to be more noticeable to our politicians.

Midnight is still too close for comfort.

Yours sincerely,

Science and Public Affairs

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Founded in 1945 by Hyman H. Goldsmith and Eugene Rabinowitch

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COMMUNICATIONS

ARTICLES

COMMENT

REPORTS

BOOKS

CREDITS

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Communications

Genetic Engineering

James Danielli in his contribution to the symposium on genetic engineering ("Artificial Synthesis of New Life Forms," *Bulletin*, Dec. 1972) has rightly reminded us that, genetically, man is still a barbarian clad in the trappings of a civilization created by only a few men. Is it not also true that our civilization has often been threatened in the past and is even now by only a few men who constitute the political leadership of nations? They are the true barbarians who would destroy all life on Earth rather than compromise on matters allegedly affecting national honor or interest. The vast majority of men, as Danielli points out, are consumers rather than originators of civilization.

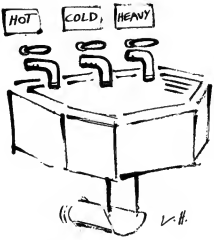
I am all for genetic engineering if it can exercise the brute in the temporal leaders of mankind—the decision-makers in our civilization. But genetic engineering, like physics, may be so much easier than politics, to paraphrase Einstein.

Rikhi Jaipal

Permanent Mission of India to the IAEA
Vienna, Austria

Energy Limits

We owe Carl Barus (*Bulletin*, Feb. 1973), as well as other critics of "Global Effects of Increased Use of Energy" (*Bulletin*, March 1972), an explanation of what were and what were not our aims in writing this paper. Our primary purpose was to uncover obvious geophysical limits to man's generation of energy. In doing this, it was necessary to make some assumptions, both with regard to the ultimate world population and with regard to the per capita generation of energy. We purposely chose extrapolations that seemed to be generous since we were concerned with upper limits. Twice the present U.S. per capita energy expenditure seemed to be adequately generous; and 15 to 20 billion people is an ultimate population that many demographers regard as inevitable. As we say in our paper, "Because we are prepared to discuss the consequences of providing for 15,000 million people does not mean that we see this eventuality as anything but a catastrophe—we accept it only because we see no practical, humane way to prevent it." We were not trying to visualize the social institutions that would obviously be needed to achieve stability in such a world,



or even to show how we go smoothly from the present world to this ultimate world.

Our analysis cannot be regarded as more than a zeroth order approximation. We believe that the zeroth order geophysical effect—global heating of the earth—is reassuringly low. The discrepancy Barus notes between our estimates of global heating according to the Stefan-Boltzmann law is indeed an error. The correct values for 15 x 10^9 and 20 x 10^9 people at 20 kilowatts (thermal) per person are 0.18°C and 0.24°C, respectively. This discrepancy in no way changes our major conclusion that man's energy production probably will not increase the temperature of the globe very much. We must concede that second order effects, such as significant changes in the earth's albedo, could change our conclusions, and we would urge that much serious attention be given to these obviously complex questions. The other bogies that have been raised—mining wastes, radioactive waste disposal, availability of nuclear fuel—seem to us not to present insoluble technical problems or unacceptable geophysical limits.

The differences between 20 billion and 15 billion persons and 12 quintillion Btu (Q) and 9 Q per year do not materially affect our conclusions. Our assumed per capita energy budget was chosen, not to permit a relaxed continuation of growth, but as essential to a drastic effort to maintain the ecological balance of the earth in the face of dwindling resources, leaner ores and accelerated development of backward areas. We found there are plenty of the basic raw materials; but increasingly more energy is needed to acquire them and to clean up afterward.

Barus is correct in noting that most of the population growth and energy increase will be outside the United States. But we see no basis for his statement that the United States can help matters elsewhere by curtailing its own productivity and imposing poverty upon America. Rather we should redirect our priorities to help other peoples control population, intensify their agriculture and utilize their resources. We believe that this requires increased personal productivity and, therefore, energy.

That we have been unable to uncover obvious geophysical limits (other than local heating) set by an energy budget of around 10 Q per year certainly does not mean that we see clearly how the underdeveloped parts of the world are going to acquire the social stability necessary to develop and, incidentally, increase their per capita energy demand. In this sense our paper,

though technologically optimistic, implies that profound social problems will certainly have to be dealt with if a society as large as we may have to cope with is ever to succeed.

R. Philip Hammond
Alvin M. Weinberg

Oak Ridge National Laboratory
Oak Ridge, Tenn.

The Numbers Game

In "The Numbers Game in Naval Strategic Balance" (*Bulletin*, Jan. 1973), Richard D. English and Dan I. Bolef correctly cite the fallacies of counting ships when addressing the "strategic naval balance." This is one of the great problems of analyzing naval affairs; we are too often misled by the "numbers game."

However, when the authors use the same approach to the numbers game which they criticize, and display an alarming lack of knowledge of basic naval matters, then the entire article becomes counterproductive.

I am particularly disturbed because the authors continually cite (and misquote) *Jane's Fighting Ships, 1971-72* as a source of data; I am responsible for much of the content of that volume.

The article begins with a discussion of budget allocations to the armed services, with the authors stating that "the Navy has been treated quite handsomely" in recent budgets. The table below presents the portion of the two budgets discussed, fiscal years 1972 and 1973, that were allocated to the armed services. These were the first budgets since the armed forces unification in 1947 wherein the Navy received the largest share of the budget; the percentage differences hardly indicate the Navy is being treated "quite handsomely."

More significant than the authors' version of the numbers game, what is wrong with the Navy receiving the largest share of a budget? The increasing vulnerability of fixed military systems and certain aspects of the Nixon doctrine, such as a reduced U.S. military profile overseas, make good arguments for increased sea-based forces if we desire to withdraw forces from foreign territory.

The authors go on to contend that Navy news releases do not mention "the vast sums to which [new] programs commit us if the projects are to reach completion." A reading of any major U.S. newspaper during the past few years reveals that many potential "billion dollar" defense programs are described in depth; as I recall, it was the Secretary of the Navy himself who stated in public that the planned CVN-70 carrier would cost $1 billion. Of course, billion dollar projects are not limited to the Navy, witness the Air Force B-1 strategic bomber and the ill-fated Army Safeguard ABM, and the main battle tank.

With respect to the specific projects mentioned by the authors as being "not often mentioned," there are many pounds of Congressional hearings available to the public that describe in considerable detail the CVN-70 carrier, SSN-688 attack submarine, and Trident/ULMS missile submarine.

The authors then proceed to discuss the Trident/ULMS project, an improved missile submarine concept to eventually replace the Polaris/Poseidon submarine forces. They quote former Secretary of Defense Laird on the high survivability of present missile submarines, and then ask if expenditures for Trident/ULMS are justified.

Obviously, missile submarines are more survivable and flexible than fixed, land-based strategic forces (bombers and ICBMs). Then why not put additional funds into your best system available? Also significant in the author's context, the initial Trident/ULMS effort will provide existing submarines with a longer-range missile, improving their survivability and flexibility as Soviet naval capabilities improve.

But in 1979, when the prototype Trident submarine becomes operational, the U.S. Polaris/Poseidon submarines will be 12 to 19 years old; a decade later, when a Trident flotilla could be at sea, the Polaris/Poseidon submarines would be 22 to 29 years old, obviously in need of replacement by newer and quieter missile submarines. These numbers represent facts that the authors carefully avoid in their discussion.

The authors next turn to the Soviet Navy, citing the excellent book *Soviet Naval Strategy* by Robert W. Herrick to conclusively state that "Soviet naval strategy is primarily deterrent and defensive. . . ." Unfortunately for the authors, Herrick completed his study in the mid-1960s, before we saw several major and significant changes in the Soviet Navy, among them emergence of many new surface warship and submarine classes that gave the Red Navy a new shape. For example, since the book was published the Soviets have completed more than one new nuclear submarine design per year. These include the Yankee and Delta nuclear missile submarine classes, the latter with a strategic missile capable of striking targets some 4,500 miles from the firing submarine—about the range of the *planned* Trident Phase I missile.

Similarly, there now is no question but the "large" ship the Soviet Union is building is an "aircraft carrier"—a ship to extend the range of Soviet naval air activities far from Red shore bases.

The authors carefully avoid any "numbers" that could be critical of their points. They note the importance of citing nuclear propulsion when comparing U.S. and Soviet warships. But the U.S. Navy has only four nuclear propelled surface ships in service. Slightly more than one per cent of the active fleet. The impact of these four ships and seven nuclear ships under construction on a comparison is limited, for in this instance lack of quantity tends to degrade quality.

	Navy (per cent)	Air Force (per cent)	Army (per cent)
FY 1972	34.56	33.77	31.66
FY 1973	35.5	33.2	31.2

Similarly, the authors address missiles on U.S. and Soviet ships while steadfastly avoiding certain numbers. Of the missiles mentioned, the U.S. Navy has Talos (our longest range missile) on four ships; it is, of course, primarily an anti-aircraft and not anti-ship missile. The cited Soviet Shaddock missile is today operational in eight cruisers and some 50 submarines.

Midway through the article the authors begin extensive citations of the annual *Jane's Fighting Ships*. I am entirely responsible for the contents of the U.S. section, and have been for several years, as stated in the book. Thus, I am confused by the constant reference to diesel-propelled aircraft carriers, and other surface warships. None of the U.S. surface ships cited as being diesel/propelled is in fact so powered.

When the authors compare *Jane's* listing of U.S. and Soviet cruisers, which they had warned was a dangerous practice, they include U.S. frigates to change an alleged 3:1 advantage claimed for the Soviet Union to a 3:1 advantage against the Soviet Union. However, they make no mention of over 18 Soviet Kashin class missile frigates which are among the world's most advanced warships and which compare most favorably to U.S. frigates.

In a most confusing manner they compare U.S. small missile/gunboat craft with the Soviet Navy's "light forces." Here we have a most ludicrous comparison of some 21 U.S. craft (four of them prototypes) with several hundred Soviet craft, and two U.S. missile-armed craft with more than a hundred Soviet missile boats, the largest of these "boats" some 200 feet long.

Of course, all naval strength comparisons which the authors provide are misleading because they do not address ship age, construction rates or ship capabilities (e.g., Soviet warships generally are more heavily armed and faster than U.S. ships).

Next, the authors contend that "NATO has never had or needed a cruise missile capability." In fact, we see frantic efforts by NATO to develop and deploy such weapons, among them the Norwegian Penguin, French Exocet, and U.S. Standard and Harpoon.

With respect to NATO, the authors state the alliance is outbuilding the Soviet Union in nuclear submarines. During the past three years the USSR has built at least 35 nuclear submarines compared to 28 for NATO; Soviet nuclear submarine construction capabilities are now more than double that of all NATO shipyards combined (or three times should the Soviets go to an around-the-clock schedule).

The errors of fact and interpretation could continue. Of significance, the 1972-1973 edition of *Jane's Fighting Ships*, available six months before the article was published, shows a continuing reduction of U.S. naval forces; the 1973-1974 issue will show even more reductions, while on the Soviet side there is stability of ship levels and considerable improvements in capabilities.

Finally, the authors must be charged with viewing naval warfare in an outdated, World War II context—situations that are no longer valid because of strategic weapons, guided missiles, electronics, nuclear propulsion, etc. True, a World War II-era tanker may not be "worth" a cruise missile in some scenarios, but today's tanker carries 10 to 20 times the cargo of a tanker built in 1945. Such a target with several hundred thousand tons of oil would be "worth" a cruise missile. At the same time, with some 25 ocean-going surface warships, over 60 submarines, and about 285 jet bombers equipped with anti-ship cruise missiles, the Soviets will aim such weapons at more targets than the 14 attack aircraft carriers now flying U.S. colors.

The increasing importance of the seas in today's political, military and economic environments demands studies and analyses of "sea power" or (my preference) "use of the sea." Hopefully, such studies and analyses will be more accurate and more meaningful than the effort by English and Bolef.

Norman Polmar

Editor, U.S. section
Jane's Fighting Ships

Response

With respect to the "display (of) an alarming lack of knowledge of basic naval matters," we wish to respond briefly to several of the items cited by Polmar:

1. Budget Allocations. We appreciate the figures cited by Polmar, since they indicate remarkably well that the Navy is being treated "quite handsomely" (35.5 per cent is equivalent to $28.7 billion of the $80.9 billion estimated expenditures for fiscal 1973).

2. Soviet Naval Strategy. Neither Robert Herrick nor we claimed that his (or our) arguments were "conclusive"—that word is Polmar's. Herrick's book was published in 1968. The characterization of the Soviet Navy, which immediately followed Herrick's quote, was made by U.S. Naval Chief of Staff Admiral E. R. Zumwalt, Jr., in a letter to Senator Proxmire dated June 2, 1972. He described the Soviet Navy as "a navy in support of a nation whose vital interests are those of a land power . . . designed to prevent the U.S. Navy from carrying out its mission." Since the primary U.S. Navy mission in the European area is to launch attacks on the USSR and its allies, this quote indicates that Admiral Zumwalt agrees with Herrick's view, not Polmar's.

3. Naval Vessel Power Plants. The two important categories of power plants for naval vessels are nuclear-powered and nonnuclear-powered. For the latter we incorrectly used the term "diesel-propelled"; we should have used the term "conventionally propelled" or, best of all, "nonnuclear propelled."

We are taken to task with respect to a number of technical factors. Polmar is concerned, for example, that we attach undue importance to nuclear propelled ships. We merely repeated the well-known advantages of nuclear power. The smallness of the U.S. nuclear surface fleet is obvious from our Table 1, in which ship types are distinguished by power plant. With respect to missiles on U.S. and Soviet ships, our purpose was to show why the numbers game tactic of

(Continued on page 48)

PRESIDENT NIXON'S 1973 REORGANIZATION PLAN NO. 1

WHERE DO SCIENCE AND TECHNOLOGY GO NOW?

The presidential science advisory apparatus, inaugurated by President Eisenhower nearly 20 years ago, has been ousted from the White House under President Nixon's Reorganization Plan. Its functions have been transferred to the National Science Foundation. How effective can the Foundation be in carrying them out? The question is a critical one for the American scientific community, for it bears directly on the future of federal support of scientific research and education and of the effectiveness of science and technology as a national resource.

In this article, Franklin A. Long, a former member of the President's Science Advisory Committee and a consultant to federal agencies, examines the implications of the reorganization plan as it affects the relationship between the scientific community and the President. He assesses the capability of NSF of carrying out the functions of the defunct Office of Science and Technology, the President's Science Advisory Committee and the Science Advisor and his deputy. Professor Long, a chemist, is director of the Program on Science, Technology and Society at Cornell University.

F. A. LONG

Two of the three proposals of the first Reorganization Plan of 1973, which President Nixon forwarded to Congress on Jan. 26, call for abolishing White House advisory bodies for science and technology. If the overall message had called for new, improved advisory mechanisms to replace the old, scientists and engineers might have been flattered by this prompt attention to their affairs. As it is, their attention is much more likely to be involved in examining just what they have lost and in speculating on the efficacy of the projected replacements.

In the White House fact sheet which accompanied this Plan, the proposed actions were summarized as:

"1. The Office of Emergency Preparedness (OEP) will be abolished and its functions transferred to other line agencies;

2. The Office of Science and Technology (OST) will be abolished and its functions transferred to the Director of the National Science Foundation (NSF);

3. The National Aeronautics and Space Council will be abolished."

The plan also announces the President's intention to ask H. Guyford Stever, current Director of NSF, "to take on the additional post of science advisor." Since it is also very widely understood that the President's Science Advisory Committee (PSAC) has been abolished (or at least made inoperative), and since Science Advisor Edward David and his Deputy John Baldeschwieler have already departed—the net effect is to eliminate any science and technology "presence" from the inner circle of the President's Executive Office. A further point is that, as science advisor, Stever will apparently normally report, not to the President, but to Presidential Assistant George Shultz; indeed, to Kenneth Dam, one of Shultz's assistants. Dam, characterized by one of his Washington colleagues as "an overworked young lawyer from the budget office," has reportedly been designated as staff contact for science and technology.

The reactions of the science and technology community to these substantial changes will necessarily involve an overall assessment of their consequences. How will the Director of NSF move to carry out his new responsibilities? What other individuals and groups are likely to become involved in any new set of operating procedures?

A first and fundamental point is that the President clearly has the prerogative to restructure the Executive Office in the way he wishes. Given this, it is highly probable that Congress will raise no formal objection, since it would seem almost self-evident that no President should be expected to maintain an advisory system which he feels is not appropriate to his needs. Specifically, if a President feels no need for a science advisory system such as represented by PSAC and the Sci-

ence Advisor who chaired it, he should surely be free to abolish it. Similarly, if Executive Branch offices such as OST and the National Aeronautics and Space Council offer no useful function to a President, he should again be free to abolish them. One might even argue that, in the interest of efficiency and economy, he really *ought* to eliminate activities for which he has no need. With respect to the current reorganization, the White House explicitly notes that "it reduces the number of employees in the Executive Office by 389 positions (from 4,250 to 3,861), and achieves an estimated $2,000,000 in budget savings." Furthermore, as the White House sees it, there are significant gains other than just the saving of money. In the words of the announcement: "It [the Reorganization Plan] transfers necessary continuing functions to line departments and agencies where they can be better performed; it streamlines the Executive Office and contributes to making it more effective and responsive to Presidential needs."

What Have They Done?

On the negative side of these projected gains, what are the losses, and for whom? To look at this, it is useful to recall what PSAC, OST, and a full-time Science Advisor have done in response to major national problems. Put another way, what have they done which went beyond day-to-day advice-giving which, at least in the case of the Science Advisor and PSAC, was the reason for their establishment?

The President's Science Advisory Committee has for some years been a vigorous group of from 15 to 20 part-time advisors, chaired by the full-time Science Advisor, and explicitly committed to working four or five days a month at their PSAC responsibilities. Meetings of the full PSAC committee involved two days a month; however, the principal work of PSAC has always been done by panels chaired by PSAC members but with the large majority of members brought in from outside PSAC. (As a typical example, during the two or three years I chaired a PSAC panel on U.S. Space Programs, the total panel size was approximately 15, of which only two or sometimes three came from PSAC itself.) The caliber of the outside members of the PSAC panels has been exceedingly high. In some measure, this must be attributed to the prestige that accompanied working with the President's Science Advisory Committee, i.e., working literally in the shadow of the White House.

All of PSAC's panels—and PSAC itself—operated as confidential Presidential advisors. The firm understanding was that PSAC panel reports normally circulated internally (within the government) were published and distributed to the outside world only in special instances and with explicit Presidential approval. Many of the PSAC panels dealt with military and intelligence programs, so that the data they examined and reports they issued were classified. On the other hand, the occasional published reports from PSAC were often of considerable influence. I recall, for example, the early report on graduate education from a PSAC panel chaired by Glenn Seaborg, as well as a much later report on world food supply from a PSAC panel chaired by Ivan L. Bennett.

The Office of Science and Technology provided support staff to these many PSAC panels. In addition, it accomplished substantial study efforts on its own initiative. Typically, OST staff members were hired because of their professional qualifications; one might be a specialist in water resources, another might be particularly knowledgeable on space research programs, and so forth.

Perhaps the most important functions of OST were its internal studies of *total* U.S. programs in specific areas of science and technology, and its programs of coordination of the separate federal efforts. The Science Advisor, who was also Director of OST, served as chairman of the Federal Council for Science and Technology (FCST), the explicit coordinating mechanism for the many science and technological programs that cut across the responsibilities of the different departments and agencies of the federal establishment.

Coordination Problem

The overview and coordinating responsibilities of OST were a natural response to the fact that particular fields of science and technology were often of importance to several quite differently oriented federal agencies. For example, some half dozen federal agencies and some scores of offices within these agencies have partial concern and responsibility for the United States efforts in water resources. Given this fragmentation, the problem of coordination is of great consequence. (I mention this particular example partly because one of my colleagues, Leonard Dworsky, who is director of Cornell's Center for Water Resources, spent 18 months in Washington on the OST staff working on just this coordination problem.)

OST through its Director also played a significant role in science-advising for Congress. Although the Science Advisor in his role as Presidential Assistant was not available to Congressional committees, he was (with some inescapable ambiguity) accessible in his capacity as Director of OST. Earlier directors, notably Jerome Wiesner and Donald Hornig, were frequently called on by Congressional committees.

The spectrum of activity in which the Director of OST and its Deputy Director were involved was wide ranging and complex. Science Advisor to the President; Director of OST; Chairman of the Federal Council for Science and Technology; Chairman of PSAC—these were the formal assignments. Of at least equal importance was the task of coordinating international programs of science, and frequently serving as spokesman and repre-

sentative for the President in international scientific affairs.

An important characteristic of the Science Advisor's Office was its capacity for rapid response. One of the lessons from the crisis caused by the USSR launching of Sputnik in 1957 was the difficulty which the government experienced in rapidly mobilizing the scientific and engineering talents of the country to respond to the crisis. A considerably less momentous (but far sharper) emergency occurred in 1965 when there was an electric blackout over most of the northeastern United States. On that occasion, the President turned to his Science Advisor, who rapidly organized an information-gathering network to keep the President abreast of developments. Lower-level national emergencies occur with greater frequency, but by no means always require the attention of a Science Advisor placed next to the President. Even so, in times of rapid technical change, the availability of a Science Advisor with an able staff ought to provide important reassurance to the President as well as to the public.

To summarize, the principal functions of the Science Advisor, OST and PSAC were these:

1. To give advice to the President and other Executive Branch personnel as requested.

2. To serve as a focal point for all of U.S. science and technology and, as viewed from outside, to serve as a communications channel for scientists and engineers into the Executive Branch of the federal government. The activities of the Science Advisor in programs of international science were an important manifestation of this focusing effort.

3. To carry on studies of problems of science and technology through PSAC and OST panels and staff activities.

4. To assume a coordinating role for programs of science and technology throughout the federal government, partly through OST staff activities and partly through support of the Federal Council for Science and Technology.

5. To evaluate on behalf of the President the many proposals with high scientific and technologic content which were recommended by the individual federal agencies. (Philip Handler, president of the National Academy of Sciences, has publicly pointed to this as the function whose loss he most mourns.)

6. To serve (through the Director of OST) as an advisor and as a communications channel to Congressional committees.

As components of the Executive Offices of the White House, OST, PSAC and the Science Advisor have all enjoyed the advantages of any "in-house" activity. Access to data is easy; reporting is direct; the level of influence, which comes from being close to the President, can be high. However, as Joel Primack and Frank Von Hippel have stressed in a recent study of the comparative effectiveness of advisory groups, there are also drawbacks to being an in-house activity. The principal drawback is an inside group implicitly accepts an oath of silence if the advice is contrary to Presidential policy. Thus, it was rumored back in 1969 that PSAC was not in favor of U.S. development of the Presidentially recommended anti-ballistic missile system. However, no formal hint of any such position was ever made public. In contrast, one of the more damning aspects of PSAC's activities, at least as far as the White House was concerned, showed up in the gradual revelation that there was an unpublished PSAC panel report in existence which contained some strongly negative remarks about the supersonic transport airplane (SST).

It has been a common rumor in Washington during the last two or three years that both PSAC and OST have been substantially less effective than in earlier years. In some measure this is bound to be the governmental and public reaction to an inside advisory group whose mentor, in this case the White House, no longer finds the advice very interesting. However, in the eyes of some critics, the situation went beyond this. Witness, for example, the remarks on PSAC and OST which appear in *Aviation Week and Space Technology* in a Feb. 5 editorial, entitled, "A Hard-Nosed Budget":

> The PSAC track record was not very bright in the last decade and Mr. Nixon apparently believes it unneces-

> sary to pay for bad advice. Similarly, the Office of Science and Technology has performed no useful function for many years and will not be missed, except by those who collected federal salaries on its staff.

To complete the indictment, this editorial goes on:

> The National Aeronautics and Space Council was brightly conceived in the early dawn of the space age but never fulfilled its announced purpose. Its abolition will detract nothing from the role of science and technology in government.

Although I would quarrel seriously with the negative view of the effectiveness of PSAC and OST, and by implication that of David and Baldeschwieler, I must agree with the final comment on the National Aeronautics and Space Council. The evidence of its ineffectiveness is persuasive, and goes back almost to its inception. The nation will not miss it.

Furthermore, even a sympathetic observer must note difficulties and awkward aspects of both PSAC and OST. The former was not entirely what its name implies, the *President's* Advisory Committee, in that its policy of definite terms of appointment for members meant that a new President was presented with a group of not-necessarily-sympathetic holdovers from a previous Administration. In some measure, this same problem entailed for OST. Also, in spite of its $2 million budget, OST could not itself staff many of the major studies for which its broad responsibilities called and, consequently, was forced to ask for help from sometimes reluctant operating agencies. As one example, in the spring of 1971 OST tried, but failed, to establish a major study of the U.S. energy program by soliciting cooperation and funds from the principally involved federal agencies.

But let me return to the abolition of the three offices which, whatever their defects, clearly have been important to the recent development and utilization of science and technology in the United States: the Presidential Science Advisor, located in the Executive Office; the President's Science Advisory Committee; and the Office of Science and Technology. What is lost in having these offices abolished and their functions transferred or eliminated?

In general terms, what we are losing is clear. Science and technology lose a channel to the White House, a loss which Philip Handler has called "extremely serious." Also lost is the coordinating and ambassadorial role of the President's Science Advisor in many fields, notably international science and technology. Finally and most seriously, the nation has lost a $2 million effort of study, overview and coordination of the internal U.S. programs of science and technology which previously was carried out by groups (OST and PSAC) which could in some measure speak with the authority of the White House. To replace these the Director of the NSF has two new assignments, so the next question is: How and to what degree can he and his organization take on these several additional functions?

What about the programs and activities which occupied the full-time Science Advisor and his deputy? Some modest fraction of their multiple responsibilities can probably be absorbed within NSF by reassignment. Certainly Director Stever, by increasingly becoming the "outside man" for NSF, should be able to discharge effectively many of the international responsibilities previously carried out by David and Baldeschwieler. However, the evident problem is: Can you possibly replace two full-time and competent people with the part-time effort of an already busy agency director, no matter how competent? One must conclude that, in the absence of major NSF expansion and perhaps internal reorganization, many of the functions carried out by David and Baldeschwieler will disappear.

Considerable concern has been expressed over the comparative remoteness of NSF from the White House, inferring that it will become much harder to insert advice on science and technology into the decision-making system. This is a common Washington worry; there is even a "law" which states that the influence of an advisory group upon decision-making in the White House decreases with the square of the distance away from the President's office. In rebuttal, one could note that the Department of Defense is physically almost the farthest removed from the White House of all the major federal agencies, and its advice seems to arrive with a high signal strength. More seriously, one strongly suspects that what principally counts in the advice-giving process is the persuasiveness of the advice as evidenced by the depth of study, the coordination with other agencies and the responsiveness to priority problems. With these in mind, let us turn to the projected transfer of the functions of OST to the Director of the National Science Foundation.

When the NSF was founded in 1950, it was assigned substantial responsibilities for an overview of science and development in the United States and for evaluating the activities of other federal agencies in scientific research. Not surprisingly, since it was a new, small and weak organization, NSF made almost no attempt to assume these evaluation and overview responsibilities; it was in some measure in response to the gap so created that PSAC and, ultimately, the Office of Science and Technology were established. In returning the functions of OST to the NSF, the nation has gone full circle. One might therefore ask whether a lack of will and competence within NSF will not plague this coordinating and overview function once again?

Over the last 20 years, however, a number of things have changed for the better. First, NSF is a very much larger agency now, with a budget of approximately half a billion dollars per year.

(Continued on page 40)

The Savor of SALT

"The strategic arms limitations agreements should, as Mr. Nixon recommended, be considered on their merits, and the weapons programs recommended by the former Secretary of Defense should be considered on theirs. Attempts to link the two—either by arguing that if we do not proceed with new weapons we will be outclassed five years hence, or that failure to proceed will weaken our international position or our bargaining ability—are perhaps questionable. Moreover, there is little merit to exchanging a quantitative arms race for a qualitative one, especially if there are prospects of controlling both types of races through future negotiations." J. I. Coffey is professor of public and international affairs at the University of Pittsburgh.

J. I. COFFEY

On May 26, 1972 the United States of America and the Union of Soviet Socialist Republics initialed at Moscow two of the most important arms control agreements of the post-war period. Taken together, the Treaty on the Limitation of Anti-Ballistic Missile Systems (ABM Treaty) and the Interim Agreement on Certain Measures with Respect to the Limitation of Strategic Offensive Arms (Interim Agreement) impose stringent controls on ballistic missile defenses, less stringent but still significant controls over land-based and sea-borne missiles, and ancillary restraints on interference with the "national technical means of verification of the other Party."

Specifically, the ABM Treaty (Article 3) precludes each Party from deploying anti-ballistic missile systems or their components, except for one system of no more than 100 launchers and interceptor missiles around each Party's national capital and one other system, similar in size, elsewhere in the country, with the proviso that this second network not overlap with and reinforce the first one. Moreover, the ABM Treaty so restricts the number and deployment of radars as to make difficult any significant strengthening or extension of these systems, rules out both multiple launchers and automatic launchers for interceptor missiles, and prohibits the development, test, or deployment of sea-based, air-based, space-based or mobile land-based ABMs. In short, the Treaty bars measures which could enable either side to protect its industry and its population against nuclear attack by the other, or which could arouse concern that it was attempting to develop a capacity to do this.

The Interim Agreement, its Protocol, and the associated understandings issued by the two parties halt the construction of additional fixed land-based intercontinental ballistic missiles (ICBMs) as of July 1, 1972, preclude the conversion of land-based launchers for light ICBMs (or of ICBMs of older types deployed prior to 1964) into launchers for heavy ICBMs of types deployed after that time, and set limits to the numbers of submarine-launched ballistic missiles (SLBMs) and modern ballistic missile submarines which each party may possess [1]. Although both sides have the right to scrap older types of ICBMs or missile submarines and to replace these with more modern submarine-launched missiles, extreme shifts are barred by the overall limitations of 950 ballistic missile launchers on submarines and 62 modern submarines for the USSR and of 710 launchers and 44 modern submarines for the United States [2].

If and as the Soviets reach the ceilings set on ICBMs and SLBMs, they would have approximately 2,500 delivery vehicles compared to 2,200 for the United States (see table). Whether or not they chose to make some of the conversions authorized, the Soviets would lead in numbers of ICBMs and of modern SLBMs. They would also be ahead in deliverable megatonnage. Moreover, they should be able to develop multiple independently targetable re-entry vehicles (MIRVs) sometime during the five years covered by the Interim Agreement. Given that the Soviet Union has 25 silos for large missiles under construction, and that both sides are permitted to increase the diameter (or the depth) of their existing silos by 10 to 15 per cent, it is conceivable that the Soviet Union could install enough MIRVs to surpass the United States in numbers of deliverable warheads—if not by 1977, then sometime thereafter. Henry Kissinger, Assistant to the President for National Security Affairs, indicated that the current U.S. lead of two to one in deliverable warheads would be maintained during the period of the Interim Agreement even if the Soviets developed and deployed MIRVs of their own [3, S9602].

These factors led a number of people to argue against the Moscow agreements on the ground that they gave the Soviets an advantage and froze the United States into a position of permanent inferiority in missiles. Furthermore, some maintained that the loopholes mentioned above could enable the Soviet Union to achieve a first-strike capability by introducing still larger missiles and by "MIRVing" these and the powerful SS-9 "Scarp" missile with numerous accurate, high-yield warheads. Since such developments could adversely

affect deterrence, if not of thermonuclear war then of lower levels of aggression, they demanded that the agreements be rejected, or at least that these alleged Soviet advantages be offset by building up weapons systems which are not controlled—such as submarine-launched cruise missiles (SLCMs).

Conversely, there are those who argued that the limitations imposed on strategic arms did not go far enough. First, the agreements do not preclude MIRVs which, in the absence of ballistic missile defenses, may be unnecessary, and which can have an undesirable impact both on strategic stability and on the arms race between the Soviet Union and the United States. Second, the agreements do not constrain measures to improve the thrust, the accuracy, or even (to some degree) the size of missiles, thereby encouraging a qualitative arms race. Third, the agreements do not even mention bombers, do not deal either with American forward based systems (FBS) in Europe or with Soviet medium and intermediate range missiles, and do not affect anti-submarine warfare forces. In short, they contended that the agreements, far from being too restrictive, permitted the arms race to continue virtually unimpeded.

With respect to the first point made by those who maintained that the agreements did not go far enough, it is true that the absence of controls on MIRVs is a serious lack. If the constraints on ballistic missile defenses are observed—and there is every reason to suppose that they will be—then the main rationale for MIRVs has vanished: they are no longer needed to penetrate, to saturate, or to exhaust anti-ballistic missile systems. Moreover, given the thousands of intercontinental delivery vehicles each side possesses (and the thousands of other delivery vehicles which are not covered under the strategic arms limitations), multiple warheads are not needed to inflict high levels of damage. Furthermore, their continued development may lead to improvements in accuracy and in maneuverability which could make them highly effective against land-based missiles, even when these are emplaced in super-hardened silos. (One expert estimates that even with present technology accuracies of 30 meters or less can be achieved at intercontinental ranges [4].) One way in which an adversary could offset these improvements would be to MIRV his own missiles; however, as a corollary, these would become even more rewarding targets for counterforce attacks. Thus, the interaction between offensive and defensive deployments of MIRVs could lead to an escalation of the arms race, if not to a tendency to launch a pre-emptive strike in the event of a confrontation.

Against this, it can be argued that it would be hard to stop the deployment of MIRVs once these have been developed, and almost as hard to preclude their development. While it is possible to constrain the installation of MIRVs by limiting the number of missile tests and by precluding further testing of maneuverable warheads or penetration aids, these kinds of constraints would not be foolproof. Furthermore, the United States certainly sees MIRVs as offsetting Soviet advantages in number of missiles and in deliverable megatonnage, and might be very reluctant to forgo this weapon without further concessions by the Soviet Union. Conversely, the Soviets have seemingly viewed MIRV technology as an area in which they need to catch up and have, therefore, been reluctant to introduce controls. The problem is not hopeless and may, as Henry Kissinger hinted [3, S9608], be taken up in SALT-II; however, it was perhaps too complex and too difficult an issue to permit resolution during the first stage of negotiations.

Much the same can be said of the failure to impose constraints on other qualitative improvements, which could be extraordinarily difficult. First, it would not be possible, without the most intensive and intrusive inspection, to uncover changes in guidance systems which would improve missile accuracy or alterations in rocket engines which would increase their thrust. Second, it is feasible, as the Soviets have demonstrated recently, to install somewhat larger missiles in silos built for smaller types—another move which would be difficult to preclude. Third, one cannot freeze indefinitely technological developments, so that upgrading will go on continuously, even as missiles are checked out or refurbished. Thus, the United States probably did well to rule out increases in the number of large size weapons, and to limit enlargements of missile silos which would reduce the scope and magnitude of qualitative improvements in ICBMs.

As for bombers, these are much harder to control than are missiles. Even medium range bombers can, if refueled by tanker aircraft, strike at intercontinental targets. How then does one define a strategic bomber, or differentiate it from a fighter plane with a 1,600 mile range? Efforts to limit aircraft capable of delivering nuclear weapons to the United States or to the Soviet Union would immediately open up all kinds of other issues, including the one of whether and how to count the Soviet flotillas of medium bombers, the forward-based systems maintained in western Europe by the United States and its allies, and the bombers in the hands of France and Britain. The United States has more heavy bombers than does the Soviet Union and its planes are qualitatively superior, are of longer range, and carry greater payloads. Under these circumstances, not limiting bombers may have seemed like a desirable offset to the Soviet lead in ICBMs and SLBMs—as well as one way of achieving Air Force support for the agreement to limit strategic offensive arms.

The technical arguments against attempting to constrain anti-submarine warfare (ASW) forces are even stronger: (1) These forces include various elements, such as airplanes, ships, helicopters and shore installations, in a complex network of

relationships [5]. (2) Some of these elements play a significant role in fleet anti-submarine warfare capabilities; to constrain them might be to downgrade the protection of surface vessels against increasingly numerous and technically more advanced Soviet attack submarines. (3) Some components of ASW forces have multiple uses, as do helicopter carriers and destroyers, which can be used in amphibious operations. (4) Constraining anti-submarine warfare forces immediately involves one in questions concerning the effects of geographic asymmetries, the capabilities of detection devices, the relative utility of submarines as counterforce weapons, and a host of equally difficult issues. Thus, whatever the theoretical desirability of limiting ASW forces, and thereby

U.S. AND SOVIET STRATEGIC NUCLEAR FORCES, 1972-1977

Offensive Weapons Systems	1972 Operational or Under Construction		1977 Programmed or Potential		1977 Under SALT	
	U.S.	USSR	U.S.	USSR	U.S.	USSR
ICBMs	1,054	1,618 [a]	1,054	2,500(?)	1,054	1,618/1,408 [b]
SLBMs	656	740 [c]	656	1,200(?)	656	740/950
Intercontinental Bombers [d]	457 [e]	140	321 [f]	140	321 [f]	140/140
TOTAL	2,167	2,498	2,031	3,840(?) [g]	2,031	2,498
Deliverable Warheads	6,562 [h]	2,281 [i]	13,193	4,104 [6,436] [j]	13,193	2,603 [3,967] [k]
(Independently Targetable)	(5,792) [h]	(2,281) [i]	(12,773)	(4,104) [6,436]	(12,773)	(2,603) [3,967]
Deliverable Megatonnage [l]	4,940	11,900	3,750	19,800 [14,000]	3,750	12,160 [8,580]
Megaton Equivalents [l,m]	4,620	5,700	4,670	9,900 [9,800]	4,670	(6,200) [6,100]
Defensive Weapons Systems:						
ABM Interceptors [c]	0	64	300(?)	1,000	200	200

Source: The force postures and numbers of deliverable warheads in this table are derived primarily from "The Defense Monitor," Vol. 1 (3) (Washington, D.C.: The Center for Defense Information, July 1972), except for the force loadings for bombers. These, together with warhead yields for missiles, are based on figures cited in Coffey [11, pp. 180-81], utilizing the lower yields for bombs, which indicates why the resultant calculations of megatonnage differ so sharply from those in "The Military Balance, 1972-1973" (London: The International Institute for Strategic Studies, 1972), pp. 84-86. The source for 1977 projections of Soviet forces was the "Hearing on the Military Implications of the Treaty" [6].

(a) Of these, only 1,527 missiles were emplaced at the time of the Moscow agreements; 66 silos for "light" missiles and 25 for "heavy" ones being yet unfilled.

(b) These figures represent projected numbers or conversions of authorized vehicles.

(c) The figure of 740 SLBMs (which includes 30 on the 10 H-class missile submarines) represents an agreed compromise between the U.S. estimate of 680 and the Soviet claim of 768 SLBMs operational or under construction. The number operational at the time of the Moscow agreement was 580.

(d) Intercontinental bombers are not limited by the SALT agreements; neither are types of defensive weapons other than ABM systems.

(e) This is the figure listed by the Department of Defense under the category "heavy bombers." If one adds the 72 FB-111-A medium bombers (66 of which are in operational units), the total would rise to 549; if one subtracts the 65 B-52s in active storage and those B-52s and FB-111-As in training and testing programs, the figure drops to about 466—of which as many as 150 may be engaged in conventional operations in Southeast Asia.

(f) Consisting of 255 B-52 G-H heavy bombers and 66 FB-111A medium bombers in operational units. Some additional planes would be available for test and training purposes, and about 200 B-52 D-F bombers would be in active storage.

(g) The numbers of launch vehicles are based on the projections for each type of vehicle made by Adm. Moorer [6, p. 144], even though this results in total forces larger than those cited elsewhere. (See, for example, Alton E. Quanbeck and Barry M. Blechman, "The Arms Accords: Everyone Gains," The Washington Post, June 4, 1972.) It is assumed that all 500 SS-9 ICBMs and 754 of the 1,200 SLBMs carry MIRVs (see also note j).

(h) The official DOD figure is 5,598 operational independent nuclear warheads, based on a somewhat different warhead loading for bombers. The total given here reflects the triple (but not independently targetable) warheads on the 21 submarines still equipped with Polaris A-3 SLBMs. Neither the DOD figures nor those given here include the warheads on some 10 submarines undergoing conversion from Polaris A-2 and A-3 to Poseidon.

(i) The official DOD figure of 2,240 reflects both a slightly lower warhead loading for bombers and a discrepancy between the number of ICBMs and SLBMs considered operational in July 1972 (2,130) and the number of warheads attributed to these (1,970).

(j) Figures in brackets indicate totals if Soviet vehicles are MIRVed.

(k) I have followed "The Defense Monitor" (Table IV, p.9) in allowing for MIRVs on all SS-9 or larger ICBMs and on the SS-N-8 SLBM, giving to the former three 5 megaton warheads and to the latter three 200 kiloton warheads. Although this is highly arbitrary, the resultant totals are not far from the figure of 3,700 independent warheads which the U.S. Senate Foreign Relations Committee estimated could result from reasonable assumptions of intensive efforts by the Soviets. (See the "Center Log," No. 45, August 11, 1972 [Washington, D.C.: The Center for Defense Information].)

(l) Approximations.

(m) Megaton equivalents are computed by taking the two-thirds power of the yield of any nuclear weapon (Y-2/3) and determining the corresponding percentage of the yield of a 1 megaton warhead. Thus, this figure gives a crude comparison of the capabilities of each side to inflict damage in a nuclear exchange.

securing the sea-borne component of the deterrent, controls over ASW must rank far down the list of negotiable items.

What has been said so far suggests that the two types of limitations imposed on strategic armaments are reasonable, if not perfect, and that those who were dissatisfied with the agreements reached were perhaps asking for perfection in a world where this is not attainable. What, however, about the arguments of those who opposed these agreements, on the ground that they would perpetuate a Soviet advantage and freeze the United States into an inferior posture? First, as various officials have pointed out [6], and as the table shows, the balance over the next five years with arms control will be more favorable for the United States than that without it—largely because the Soviet Union has on-going programs for the construction of additional ICBMs and missile submarines and the United States does not. Second, despite the Soviet lead in missiles, the United States retains significant advantage in numbers of bombers and an even more significant edge with respect to the number of deliverable warheads—thanks in part to its technical lead in MIRVs. Furthermore, the freeze on U.S. (and Soviet) forces is very slushy, as shown by the request of ex-Defense Secretary Melvin R. Laird for funds for the development of a new bomber and for the accelerated procurement of the advanced missile submarine Trident system. (In fact, the freeze will check the momentum of Soviet weapons programs and give the United States time to start up its own, and thus may be disadvantageous to the Soviets.) Finally, as the former chief U.S. negotiator, Gerard C. Smith, warned, unless agreement is reached on measures which would within the next five years "constrain and reduce" threats to the survivability of its strategic retaliatory forces, the United States may withdraw from the treaty limiting rival anti-ballistic missile systems. Thus, there is no indication that the United States will be placed in—or will permit itself to remain in—a position inferior to that of the Soviet Union.

Even if the United States should do nothing, it is hard to see how the Soviet Union could by 1977 gain a meaningful strategic advantage. Although John S. Foster, Jr., Director of Defense Research and Engineering, estimated that the Soviet Union could, within 3 to 5 years, pose a threat to U.S. land-based strategic forces, he also testified that they have had to scrap their prototype system for multiple warhead missiles (which was potentially convertible into a vehicle for launching MIRVs) and start all over again [7]. According to Secretary of Defense Laird, it will take the Soviets two years from the time they first test MIRVs to the time they can begin to deploy them [8], and thus still longer to have them in quantity. Moreover, given the limitations imposed on the number of SS-9s, the Soviets will also have to improve the yield-to-weight ratio of warheads on this missile, its accuracy (as well as that of the SS-11 missile) or both. Hence, while the USSR could conceivably launch one or more megaton-yield warheads against each U.S. ICBM, it is questionable whether such an attack could destroy even this component of the U.S. deterrent. In fact, the Chief of Staff of the Air Force, General John D. Ryan, expressed confidence that Minuteman "can survive an attack against U.S. strategic forces in more than adequate numbers to maintain its deterrent effectiveness, both now and in the future" [9].

Furthermore, as General Ryan and other officers pointed out, land-based missiles are but one element of U.S. retaliatory forces. The MIRVing of Soviet missiles is unlikely to affect American bombers, whose safety is based on widespread deployment, mobility and rapid take-off when warning of an attack is received. To bombers, submarine-launched ballistic missiles are likely to be more threatening than even MIRVed ICBMs; however, it would be difficult for the Soviets to position their submarines for depressed trajectory launches (which alone could hope to catch many bombers on the ground), and even more difficult to coordinate such launches with attacks on ICBMs by missiles originating in the USSR. Finally, those U.S. missile submarines at sea—normally amounting to 60 per cent of the 41-boat Polaris/Poseidon Fleet—would be unaffected by a Soviet first-strike. When we consider that one submarine armed with Poseidon missiles can launch 160 warheads more devastating than those dropped on Hiroshima or Nagasaki, and that two such submarines could deliver more warheads than there are cities of over 100,000 people (even after allowing for attrition by the missile defenses around Moscow), it is obvious that the Soviets could not hope to erode U.S. retaliatory capabilities to a degree that would make a "first-strike" a feasible course of action.

This suggests arguments about the military implications of imbalances in numbers of strategic weapons are largely meaningless. First, the ultimate targets of these weapons are not other weapons, but installations and populations which are highly vulnerable to nuclear attack. Second, the amount of damage which even one weapon can inflict is almost unimaginable; a single 20-megaton warhead dropped on my home city of Pittsburgh would kill over one million Americans—more than have been killed in all our wars to date, including the Civil War [10]. Third, the numbers of weapons available to each side are so great that both the United States and the Soviet Union are likely to run out of profitable targets long before they run out of deliverable warheads. Certainly, the United States, despite asymmetries in force postures, in weapons systems capabilities, and in geography, could inflict levels of damage which would eliminate the USSR as a functioning state and put an end to Soviet society in its current form; if this does not deter attack it is hard to see what would.

It is noteworthy that Administration spokesmen, in calling for new weapons programs, have done so not on the basis that these are needed to maintain the deterrent but on other grounds. For example President Nixon, in his press conference of June 22, 1972, maintained that if the Soviet Union is allowed to gain a substantial lead over the United States, this would be "an open invitation for more instability in the world and an open invitation in my opinion for more potential aggression in the world, particularly in such potentially explosive areas as the Mid-East." Adm. Thomas H. Moorer, Chairman of the Joint Chiefs of Staff, testified that failure to go forward with "mandatory" developments needed to maintain modern deterrent forces would be ascribed by the Soviets "not to good will, but to a failure of will, not to our confidence, but to our weakness." President Nixon argued that failure to build weapons which were not controlled by U.S.-USSR agreements would leave the United States in an inferior position at the end of the 1970s, thereby placing in jeopardy "our foreign policy and our commitments around the world." Finally, not only Mr. Nixon and Mr. Kissinger [3, S9599] but also ex-Secretary of Defense Laird [8, p. 4] have maintained that programs such as those for the Trident missile submarine and the new B-1 bomber are essential to success in future negotiations on the limitation of strategic armaments.

Admittedly, the ability to inflict damage is not the only component of deterrence, even when one is seeking to ward off a direct nuclear attack. It is perhaps even less important when one is seeking to deter other forms of aggression or to influence political behavior. In such instances, the country possessing an advantage may still attach psychological importance to this advantage and expect its adversary to back down. Conversely, the country which is at a disadvantage may be more reluctant to take actions which run the risk of nuclear war. In practice, however, these advantages are largely illusory and may not count as much as perceptions of interest in a given issue and of commitment to a given outcome—which is why Europeans are so insistent on maintaining a high level of American troops in Europe. Moreover, once levels of damage have gone beyond a rather low point further increases may not significantly affect decisions; for example, 52 defense analysts assessing the outcomes of a hypothetical U.S.-Soviet confrontation in the Middle East estimated that the responses of the United States (and the Soviet Union) to threats and provocations would not change appreciably even though the number of people who would be killed in a nuclear exchange varied from 20 million to 100 million [11]. Whatever the relative differences in strategic capabilities, both the potential consequences of a nuclear exchange and the possibility that actions taken may precipitate such an exchange serve to inhibit direct challenges to national interests.

As far as political instabilities are concerned, the belief that these will vary with changes in the strategic balance presumes that the deterrent is the sole—or at least the most important—factor affecting Soviet behavior. Yet, as the Moscow Summit demonstrated, important agreements tending to stabilize U.S.-Soviet relations were reached despite recent and marked increases in Soviet nuclear capabilities. Moreover, these agreements are only part of far-reaching endeavors to improve the political climate, endeavors which are reflected not only in the statement on "Basic Principles of Relations between the United States of America and the Union of Soviet Socialist Republics" [12, p. 943], but also in the four-power agreement on West Berlin and in the *Ostpolitik* of the Federal Republic of Germany. This climate is only one among many factors affecting not instability, which is endemic, but the willingness to exploit this instability. Other factors are the domestic opposition to foreign adventures, which exists in the USSR as well as in the United States; the adverse effects of attempts at exploitation on other goals, such as *detente* in Europe and increased East-West trade; the economic costs of such attempts both directly (as in aid to Egypt) and indirectly (as in expenditures for defense programs); the willingness of other countries to be used by the great powers in their search for "advantages"; and the possibility of using them to this end, considering both indigenous opposition and the local military forces of the other great power. (For example, the U.S. Sixth Fleet is still

larger and more powerful than the Soviet Mediterranean Flotilla—which would, moreover, in time of war have to cope with British, French and other NATO naval forces in the Mediterranean Sea.)

Bargaining Chips

National will and the credibility of national commitments are not manifested solely by decisions on weapons programs or by the relative status of one component of the military balance; were that the case, belief in the United States would long since have eroded. Instead, estimates of the probability that the United States will respond to pressures on its allies or to actions against its interests are based on much more complex and subtle judgments. They involve assessments of the ability of the United States to differentiate between basic interests and peripheral ones, since the likelihood of a response rises sharply with the level and the nature of the interests at stake. They involve consideration of circumstances under which the United States would have no choice save to stand fast—as it certainly would in the case of Soviet aggression in Europe or in the Middle East. They reflect assessments of the extent to which American interests are parallel with those of the allies—with some of the latter perhaps tending to see failure to accommodate to their views as reflecting a lack of will rather than a difference of opinion. Even if one grants that in the present state of the world unilateral reductions in defense programs might have an adverse effect on allied opinions, it does not follow that unilateral increases will have a favorable one. If the build-up of military forces were the only way of demonstrating national will, Hanoi would long since have had to quit the war.

This brings us to the argument that ongoing weapons programs are essential as bargaining chips in future negotiations on the limitation of strategic armaments. It must be admitted that the Soviet Union may not be as amenable to agreements calling for cuts in its own forces if it feels that the United States is going to make unilateral reductions anyway. However, other factors will influence Soviet decisions on arms control measures, including the costs of maintaining current forces, priorities in the allocation of resources, and its own desires both to achieve *detente* and to create the impression of working towards accommodation. More importantly, the Soviets have consistently opposed the concept of "bargaining from strength" which is implicit in this line of argument—and in some cases made explicit by its proponents. In fact, as Henry Kissinger has indicated, the USSR agreed to place constraints on submarine-launched ballistic missiles even though the United States was "in a rather complex position to recommend a submarine deal, since we were not building any and the Soviets were building eight or nine a year, which is not the most brilliant bargaining position" [12, p. 935]. Finally, to start weapons programs which are subsequently stopped can be very expensive; it is now estimated that it will cost the United States $8.9 billion to build the two missile defense complexes authorized by the Moscow agreements. It is possible that some of the same effects on negotiations could be achieved by placing money for weapons systems in escrow, releasable if no agreement is reached within a given time. Alternatively, it is conceivable that the negotiating position could be strengthened without undue expense if only critical and long lead time items were funded, so that the costs of subsequently cancelling or reducing these would be minimal; in fact, Secretary of Defense Laird has hinted that this would be an acceptable approach.

Conclusions

All this leads to several conclusions.

• The strategic arms limitations agreements should, as Mr. Nixon suggested, be considered on their merits, and the weapons programs recommended by the Secretary of Defense should be considered on theirs. Attempts to link the two—either by arguing that if we do not proceed with new weapons we will be outclassed five years hence, or that failure to proceed will weaken our international position or our bargaining ability—are perhaps questionable. Moreover, there is little merit to exchanging a quantitative arms race for a qualitative one, especially if there are prospects of controlling both types of races through future negotiations.

• We should proceed rapidly with SALT-II, which started in the fall of 1972. While the items on any agenda for future arms limitations are likely to be numerous, there are several measures that would seem particularly important and urgent. The first measure would be to trade off reductions in U.S. bomber forces (which may in the long run be reduced anyway simply because of cost) for cuts in the larger Soviet missiles, which could be upgraded or converted into modern SLBMs. A companion measure should be to trade off older Soviet submarines (and perhaps a slow down in the construction of newer ones) for some of the 10 Polaris boats which the Administration has proposed replacing with the newer and larger Trident missile submarine. Taken together, these two measures would bring about actual reductions in levels of weapons, preclude the Soviets from achieving significant advantages in numbers of modern missiles, and head off qualitative improvements on both sides. A related measure would be to preclude significant alterations in the ability of either side to conduct counterforce strikes, perhaps by limiting the number of missile tests (thereby inhibiting increases in missile accuracy) or perhaps by restricting the deployment of MIRVs. The United States has reportedly announced that if the Soviet Union started installing MIRVs on its larger missiles, the United States would consider the arms control agreements null and void [13]. Given this, and the United States lead in MIRV

technology, the Soviet Union may be amenable to arrangements which would halt the American deployment of MIRVs, even if this means giving up such weapons itself, or at least not attempting to develop individually-targetable warheads for their SS-9 missile. Although no one can be sanguine about the possibility of constraining MIRVs, the attempt to do so is worth making.

• The United States should defer a decision to proceed with the production of the Trident system until the outcome of SALT-II becomes clearer. As an interim measure, it could continue with the development of ULMS-I (a missile of about 4,000 miles range which could be installed in the launch chambers fitted with Poseidon SLBMs) and with research on a successor to the current missile-submarines, should this prove necessary. Or as an alternative, and as a concession to those who are concerned about bargaining positions, the Congress might appropriate funds for long lead time items which could be used in other types of nuclear submarines and put these monies in escrow for a period of time, say 18 months. If, by that time, reductions in forces had not been made, the United States could restart or accelerate the Trident program, should this still be deemed necessary. A number of experts who have argued against the Trident, however, have done so on the ground that this program may constitute a premature commitment to a submarine of lesser utility than other models which might be needed in the 1980s. In any case, the decision to proceed with a new missile-submarine should be based not on the number of boats the Soviets have, or on how old the American ones happen to be, but on the ability of the Soviet Union to sink them—which, according to the Chief of Naval Operations, Admiral Elmo R. Zumwalt, Jr., is precisely zero [6, p. 536].

• The program for the deployment of the B-1 bomber should be looked at not in terms of a demonstration of will, or of its utility as a bargaining chip in negotiations, but essentially in terms of its military-technical utility. Some of the B-52s—perhaps the less versatile D and F models—will ultimately be retired in any event, or may be traded off for reductions in Soviet ICBM forces, as suggested previously. The issue then becomes one of whether the more usable B-52 Gs and Hs, whose effective life will certainly extend into the early 1980s, need to be replaced. Given the versatility of bombers, their ability to make multiple attacks and, above all, the fact that these attacks are of a different nature from those made by missiles, and cannot be countered in the same way, a bomber component of the deterrent will probably continue to be desirable. However, whether that requires building a new bomber at this time, and whether that new bomber has to be the B-1, are questions which are at least moot.

In short, this analysis suggests that there is no urgent need for the United States to procure more sophisticated bombers, larger missile submarines, or cruise-type missiles. In fact, as Administration spokesmen have pointed out, the current understandings curb on-going Soviet programs which might have required the development of these (and other) weapons. This does not mean that new missile submarines or more advanced bombers may not ultimately be necessary, only that decisions to build them do not have to be made at this time. As Mr. Nixon pointed out, "it is clear that the [strategic arms limitations] agreements forestall a major spiraling of the arms race—one which would have worked to our disadvantage" [12, p. 978]. To intensify this spiral before trying to impose tighter limits on the arms race would be not only a mistake but a tragedy—since the new weapons proposed do not seem necessary either to protect American interests or to achieve further progress in the control of armaments.

NOTES

1. Interim Agreement, Articles 1-3.

2. Protocol, Interim Agreement.

3. U.S. Congress, Senate, Briefing by Henry Kissinger, June 19, 1972, Cong. Rec., S9599-S9608.

4. D.G. Hoag, Ballistic-missile Guidance, in "Impact of New Technologies on the Arms Race," ed. by Bernard T. Feld, et al. (Cambridge, Mass.: The MIT Press, 1971), p. 100.

5. For an excellent discussion of weapons, tactics and the problems of anti-submarine warfare, see Richard L. Garwin, Antisubmarine Warfare and National Security, "Scientific American," 227 (July 1972), 14.

6. See, for example, statement by Henry Kissinger, "Weekly Compilation of Presidential Documents" (Washington, D.C.: Office of the Federal Register, National Archives and Records Service) May 27, 1972, p. 934; and U.S. Congress, Statements by Melvin R. Laird, Adm. Thomas H. Moorer, and Adm. Elmo R. Zumwalt, Jr. at "Hearing on the Military Implications of the Treaty on the Limitations of Anti-Ballistic Missile Systems and the Interim Agreement on Limitation of Strategic Offensive Arms" before Armed Services Committee, June 6, 20, 22, 28 and July 18, 19, 21, 24, 25, 1972 (Washington, D.C.: GPO, 1972).

7. U.S. Congress, Senate, Statement by John S. Foster, "On the Safeguard—Site Defense of Minuteman" before Armed Services Committee, March 21, 1972.

8. U.S. Congress, Senate, Statement by Melvin R. Laird, "Hearing on the Military Implications of the Treaty," p. 41. The United States took 2-1/2 years to go from test to initial operational deployment—and then only of one squadron of Minuteman-III ICBMs.

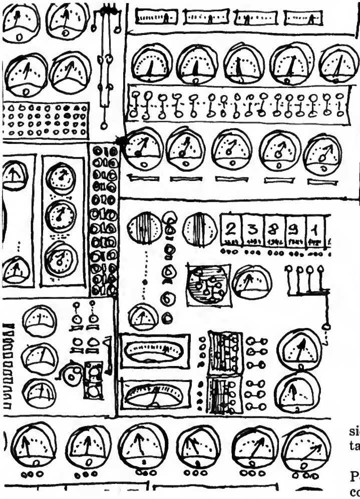
9. U.S. Congress, Senate, Statement by Gen. John D. Ryan, "Hearing on the Military Implications of the Treaty," p. 459. In this view he was supported by Gen. Bruce Palmer, Jr., Acting Chief of Staff of the Army, who indicated that a minimum of 200 Minuteman ICBMs would survive a Soviet first strike.

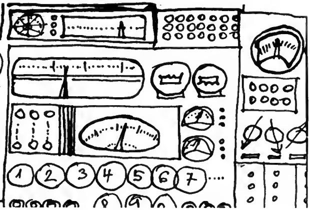
10. Pittsburgh Study Group for Nuclear Information, "The Effects of Nuclear War on the Pittsburgh Area" (Pittsburgh, Pa.: The Study Group, 1962), Table 8.

11. J.I. Coffey, "Strategic Power and National Security" (Pittsburgh, Pa.: University of Pittsburgh Press, 1971), p. 63.

12. U.S. President, "Weekly Compilation of Presidential Documents," May 27, 1972.

13. Gerard C. Smith, Washington Post, June 9, 1972. While not as explicit, Secretary of Defense Laird twice stated that the United States would view a Soviet program which threatened the Minuteman forces as violating the intent of the arms control agreement ("Hearing on the Military Implications of the Treaty," pp. 558, 564).





Meadows Unlimited or Caveat Computer

MAURICE GOLDSMITH

Letter from London

Last year at a crowded discussion meeting at the American Embassy here, I was invited to say a few words on the occasion of the first appearance of Dennis Meadows in public following the publication of *Limits to Growth.* I congratulated him on his pioneering extension of Jay Forrester's computer model World 2, expressed the hope that some critical studies would be made of his techniques and data, and suggested that innovative man might defer catastrophe as forecast by these MIT specialists.

I mention this not in self-praise, because they were obvious comments. But I felt at the time that this report, inspired by the Club of Rome's project on the predicament of mankind—which arose out of a meeting in April 1968 in the Accademia dei Lincei in Rome at which I was present, although my links with the Club have been tenuous since—was "contrary to human nature." Of course, I could not justify that, but my feelings of unease were reinforced when I observed that support for the implementation of the *Limits to Growth* conclusions came largely from what were vaguely authoritarian-type thinkers and groups.

Now, from that well-esteemed team, the Science Policy Research Unit at the University of Sussex, comes a critique of Forrester's *World Dynamics* and *Limits to Growth*—the first scientific appraisal of the structure and assumptions of the MIT models (and which incidentally sets out clearly the justifications for my uneasiness).

Let me remind you that both *World Dynamics* and *Limits to Growth* attempt to provide a general model of the future of the world by bringing together forecasts of population growth, resource depletion, food supply, capital investment and pollution. Meadows accepts the Forrester assertion that computer models have certain inherent advantages over mental models, because although the human mind can identify the main features of problems so complex as those they are dealing with, it cannot estimate well the dynamic consequences of the way the parts of a system will interact. Their actual computer models consist of sets of programmed equations; they are also represented in Forrester's formal Systems Dynamics notation.

Meadows' World 3 model has the same basic structure and underlying assumptions as World 2, but there are about three times as many mathematical equations and much wider use of empirical data. "Standard runs" for both models predict catastrophe by the year 2100. This is typical of a system in which exponential growth is colliding with a finite limit.

What requires to be done? Forrester and Mead-

ows urge us to act now to alter the growth trends, and to establish a condition of ecological and economic stability sustainable far into the future. This would include restricting population growth, fixing world average industrial output per capita at 1975 levels, shifting consumption away from material goods into services such as education.

Although Forrester points out clearly that his model is not precise enough to show the actual form of collapse, this does not prevent him from recommending certain policy actions. Meadows, too, has great confidence in his model—"already sufficiently developed to be of some use to decision makers." Nor does he expect his "broad conclusions to be substantially altered by further revisions."

Alas, the final sentence in the Sussex critique reads: "What, then, remains of Forrester's and Meadows' efforts? Nothing, it seems to us, that can be used immediately for policy formation by decision makers; a technique, one among several, systems dynamics, of promise which needs improvement; but above all a challenge to all concerned with man's future to do better."

The Sussex critique is the work of a multi-disciplinary team: H.S.D. Cole, mathematical physicist; T.C. Sinclair, health physicist and nuclear engineer; K.L.R. Pavitt, engineer and economist; W. Page, social psychologist; A.J. Surrey and A.J. Bromley, economists specializing in energy; R.C. Curnow, statistician; P. A. Julien and C. M. Cooper, economists; P.K. Marstrand, biologist; H.G. Simmons, political scientist; C. Freeman, director of the Science Policy Research Unit; and Marie Jahoda, social psychologist and coordinator of research.

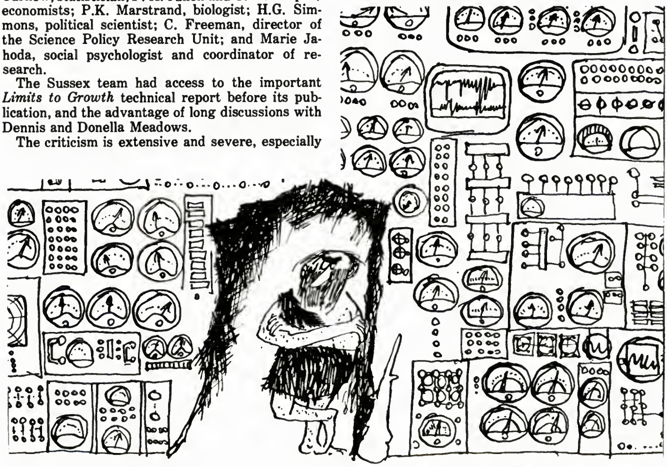
The Sussex team had access to the important *Limits to Growth* technical report before its publication, and the advantage of long discussions with Dennis and Donella Meadows.

The criticism is extensive and severe, especially of the structure and assumptions of the MIT models, although the Sussex authors agree about the urgency of many of the social problems raised, and praise the courageous and pioneering attempt of the MIT model-makers. The question posed in relation to each subsystem and the model as a whole is: How far do the assumptions made correspond to what is known about the real world before 1970, and to what might be assumed plausibly about the world's future development from then onwards? The MIT models do not satisfy those requirements. They are criticised not for the lack of data, but for erecting an elaborate theoretical structure and such sweeping conclusions on a data base that is so thin.

Computer Fetishism

Forrester and Meadows are warned, also, of the dangers of "computer fetishism," in which the computer model is endowed with a validity and an independent power which transcends the mental model from which it is derived. This is a significant feature of the Sussex critique: Beware! Beware! The apparent detached neutrality of a computer model is as illusory as it is persuasive.

Assumptions about the workings of a social system are coloured by the attitudes and values of those concerned. Because they may hide possible sources of bias, computer models should be regarded as an integral part of political debate. In developing a critique of a computer model, more



is needed than to look at the structure or to conduct mathematical tests. The "more" is examination of the underlying assumptions.

On this ground there is serious indictment of the MIT model makers. They are charged with making assumptions about relationships and estimates about data which are matters of judgment, and not of fact or mathematics. They have concentrated on physical limits to growth and omitted changes in value. They have chosen to be unconcerned with politics, with social structure, and with human needs and wants. Meadows knows that exponential growth can be influenced by action, but this knowledge has not been built into his model. Innovation, the resourcefulness of men, can change the physical properties with which the model deals. If man is introduced as the sixth variable into thinking about the world and its future, this may change entirely the nature of the debate.

How do the two models stand up to critical examination? World 2 is unsatisfactory as a forecasting device, as is World 3 despite its extra complexity, which gives a false note of sophistication and exactitude. "Perhaps the best result one could hope for from any model like the two discussed here is that they contain the seeds of their own destruction—and of their replacement by superior models. Worlds 2 and 3 may prove to be useful stepping stones to better models."

On technical grounds, the *World Dynamics* and *Limits to Growth* argument depends on what negative feedbacks are introduced into systems dynamics and with what power. The Sussex critique points out for instance that World 3 has no feedback loop between the market, technology and economically exploitable resources; nor the social, economic and technological feedback mechanisms which would operate to keep pollution at an acceptable level. Slight changes in the assumptions in the subsystems are sufficient to secure that the adaptive processes can remove the threat of collapse from particular causes. In any model of the world system, continuous technological improvement ought to be included.

I cannot omit a most important part of the critique which deals with the strong technocratic tendencies inherent in these models. The Forrester-Meadows view of the nature of the social system fits in well with those impatient of the democratic political process with its necessary compromises and halfway measures. How beautiful—and how dangerous—is the utopian vision of a stable state in which anxieties and conflicts arising from the "rat race" and economic growth will have gone, in which man will be able to develop his spiritual side, and all will be clean and beautiful. "This is not to denigrate the beliefs of the Forrester-Meadows school in any sense; rather, it is to suggest that they, too, despite the surface appearance of scientific neutrality and objectivity, bring us a message which can only be fully understood in the context of their own beliefs, values, assumptions and goals."

Meanwhile, from the Club of Rome comes an urgent statement that *Limits to Growth* is a report *to* the Club, not *of* the Club. It is not a statement of the Club credo, but "a first hesitant step towards a new understanding of the world." The Club insists that it is not a group advocating zero growth, or interested only in the problems of industrialized countries, or a political organization, or a body devoted to public propaganda for change. It has a catalytic function and has performed that well already, as demonstrated by the response to *Limits to Growth.*

Next steps under consideration are to promote The World Forum, the equivalent of the Club at a political level for senior ministers, and to welcome the spontaneous emergence of national Club of Rome groups as have arisen in Japan and the Netherlands.

It is clear that the Club of Rome and its approach will survive the critics, but—like the new Gaullist government in France—in a much-tempered form.

Note: The Sussex critique appeared in March and May 1973 issues of *Futures* magazine, and in the book *Thinking About the Future* (London: Chatto & Windus, 1973).

The BEIR Report: A Focus on Issues

In 1969, a national controversy erupted on the issue of the safety of federal radiation protection standards for nuclear power reactors. Two scientists, Arthur R. Tamplin and John W. Gofman, of the Atomic Energy Commission's Lawrence-Livermore Laboratory, issued a manifesto charging that the AEC standards allowed nuclear power facilities to emit 10 times more radiation than was safe. They called for a 10-fold reduction. AEC officials responded by denouncing the manifesto as erroneous and unscientific.

Last winter, the National Academy of Sciences-National Research Council broke a long silence on this controversy. It reported the results of a review of the standards and the federal radiation protection guide on which they were based by its Advisory Committee on Biological Effects of Ionizing Radiation (Bulletin, *March 1973). The report clearly supported Tamplin and Gofman.*

In this comment, Dr. Tamplin, who, along with Dr. Gofman, was severely critized by superiors for daring to challenge the agency's standards, reviews the impact of the BEIR report and the background of the controversy.

ARTHUR R. TAMPLIN

On January 28, 1970, Robert H. Finch, then Secretary of Health, Education and Welfare, closed a letter to Senator Edmund S. Muskie with the following:

> Drs. Gofman and Tamplin have raised the question of whether the present FRC guidelines are still acceptable. In the past ten years, since the formulation of the FRC basic guides, sufficient additional information has developed from epidemiologic studies and animal experiments so that a reevaluation of such guidelines is believed to be warranted.
>
> In view of our concern with the potential hazard of ionizing radiation in the environment, and as chairman of the FRC, I am recommending that the Council institute a careful review and evaluation of the relevant scientific information that has become available in the past decade. I am recommending that this reevaluation provide, as definitively as possible, estimates of the risks associated with low levels of environmental radiation as a basis for review of the adequacy of current FRC guidelines as applicable to projected radiation levels. Based on projected exposure classes of radiation sources, such as nuclear power reactors, other peaceful uses of nuclear energy, and radiation from consumer products would also be considered.

FRC refers to the Federal Radiation Council. The FRC was subsequently abolished and its function assigned to the Environmental Protection Agency (EPA). In the meantime, the FRC had contracted with the National Academy of Sciences-National Research Council (NAS-NRC) Advisory Committee on the Biological Effects of Ionizing Radiations (BEIR) to conduct the review called for by Secretary Finch. In November 1972, the committee submitted its report—BEIR report—to the EPA.

Before discussing the BEIR report, it appears worthwhile to put it in historical perspective. In November 1969, John W. Gofman and I testified before the Senate Subcommittee on Air and Water Pollution chaired by Senator Muskie. We testified that the more recent data on the cancer-inducing potential of radiation were far worse than supposed when the existing radiation exposure standards were promulgated. We estimated that the allowable population exposure guide (170 millirems per year) would produce an additional 16,000 cancer deaths per year. Later we raised the estimate to 32,000. We also estimated that the genetic effects would produce a 5 per cent to 50 per cent increase in ill health and a commensurate increase in the yearly death rate. We felt that these effects were too severe and urged that existing exposure standards be reduced by at least a factor of 10.

We were immediately accused of heresy or worse by the atomic industrial establishment. On April 27, 1970, Cyril Comar, chairman of the BEIR committee, wrote Charles Dunham, head of the NAS Division of Medical Sciences:

> Radiation Protection Standards are formulated by several independent national and international bodies, namely, the NCRP [National Council on Radiation Protection and Measurements], ICRP [International Commission on Radiological Protection], and FRC. In addition, periodic scholarly reviews of pertinent data are provided by UNSCEAR [U.N. Scientific Committee on the Effects of Atomic Radiation]. Recent reviews by these groups (ICRP 1966, 1969; UNSCEAR 1964, 1966, 1969) have considered in depth essentially all of the available data relevant to the setting of standards. These bodies have found no evidence that warrants a downward revision of the basic radiation standard of 5 rems per 30 years or 170 mrems per year to the general population.
>
> Pertinent data have been under continuous review by the NAS-NRC advisory committee to the FRC. This committee has specifically reviewed the statements presented before Congressional committees and elsewhere to support the allegations referred to above and conclude that these statements contain no data that would significantly alter the base upon which current standards were established. There is no evidence available to the committee that exposure of the public will increase at a rate that would in any way justify an emergency revision of the existing standards.
>
> Because of the allegations and widespread public concern the committee feels it must plan further consideration of the interpretation of data relative to estimating risks associated with low levels of radiation exposure and the utilization of such interpretations for establishment of radiation standards.

In essence, Comar was saying that we were wrong but that it would take him two years to uncover the reasons. On January 15, 1971, the NCRP issued a lengthy report, "Basic Radiation Protection Criteria." In it the NCRP stated:

> Ultimately, realistic interpretation in various applications derives from public understanding of, and eventual approbation of, practices developed from recommendations of responsible technical bodies. In particular, it is believed that while exposures of workers and the general population should be kept to the lowest practicable level at all times, the presently permitted exposures represent a level of risk so small compared with other hazards of life, and so well offset by perceptible benefits, that such approbation will be achieved when informed public review process is completed.

Nevertheless, in June 1971, the Atomic Energy Commission (AEC) revised the design specifications for light water moderated nuclear power reactors (Title 10, Code of Federal Regulations, Part 50). These new regulations called for a hundredfold reduction in the radiation exposure limit for this type of reactor. The 170 mrem/yr guide, however, was left as a primary standard in Part 20 of these regulations.

End of Controversy

Finally, after some three years of controversy, the BEIR report concluded (contrary to the above opinion of the NCRP):

> The present guides of 170 mrem/yr grew out of an effort to balance societal needs against genetic risks. It appears that these needs can be met with far lower average exposures and lower genetic and somatic risk than permitted by the current radiation protection guide. To this extent, the current guide is unnecessarily high.

The BEIR committee estimated that the existing guide could produce between 3,000 and 15,000 cancer deaths annually and lead to an increase of 5 per cent in the ill health of the population. Although there are differences between our two estimates of the effects, I consider this as an end to the controversy.

In the summary of the BEIR report, the committee made a statement concerning the *normal day-to-day* radioactive releases from the nuclear power industry:

> Concern about the nuclear power industry arises because of its potential magnitude and widespread distribution. Based on experience to date and present engineering judgment, the contribution to radiation exposure averaged over the U.S. population from the developing nuclear power industry can remain less than about 1 mrem per year (about 1 per cent of natural background) and the exposure of any individual kept to a small fraction of background.

There was never any real controversy over this issue even in 1969. As I stated above, the AEC made a hundredfold reduction in the design specifications in recognition of this potential. However, the BEIR committee went on to state that the exposures can be

> . . . kept to a small fraction of background provided that there is: (a) attainment and long-term maintenance of anticipated engineering performance, (b) adequate management of radioactive wastes, (c) control of sabotage and diversion of fissionable material, (d) avoidance of catastrophic accidents.

These have been and still are the primary unresolved issues relative to the burgeoning nuclear power industry. I prefer the comments of Nobel laureate Hannes Alfven when he discussed these issues:

> Fission energy is safe only if a number of critical devices work as they should, if a number of people in key positions follow all their instructions, if there is no sabotage, no hijacking of the transports, if no reactor fuel processing plant or reprocessing plant or repository anywhere in the world is situated in a region of riots or guerrilla activity, and no revolution or war—even a "conventional one"—takes place in these regions. The enormous quantities of extremely dangerous materials must not get into the hands of ignorant people or desperados. No acts of God can be permitted. (*Bulletin*, May 1972.)

Alvin Weinberg, director of Oak Ridge National Laboratory, described these problems in another style:

> We nuclear people have made a Faustian bargain with society. On the one hand, we offer—in the catalytic nuclear burner—an inexhaustible source of energy . . . .
>
> But the price that we demand of society for this magical energy source is both a vigilance and a longevity of our social institutions that we are quite unaccustomed to. (*Science*, July 7, 1972.)

It must be remembered, however, that by the time Faust recognized the terrible nature of his bargain, it was too late. Unless the course of the nuclear power industry is drastically altered, we will suffer the same fate.

The BEIR committee also raised another important issue:

> The exposures from medical and dental uses should be subject to the same rationale. To the extent that such exposures can be reduced without impairing benefits, they are also unnecessarily high.

Very significant reductions in the genetically significant medical exposures can be achieved by practicing good radiation hygiene without loss of medical information. However, the official defense of the existing exposure guides has made it next to impossible to bring pressure to bear on the medical profession in order to bring about this necessary reduction in exposure. Hopefully, more rapid progress can now be made in this important area.

While I disagree with some of the technical aspects of the BEIR report, I think it is an admirable consensus of a diverse assortment of committee members. After more than two years, the study is over. The conclusion is that the existing guidelines are not safe and are unnecessarily high. It is now the responsibility of the EPA, the Food and Drug Administration, and the medical profession to bring about the necessary changes in guidelines and exposures. A lot of us will be watching them. So far as the nuclear power industry is concerned, the unresolved issues are now laid bare, and I doubt if it can survive the exposure.

Resources and Long-Range Forecasts

Waldo E. Smith

And so these (blind) men of Indostan
Disputed loud and long,
Each in his own opinion
Exceeding stiff and strong,
Though each was partly in the right,
And all were in the wrong!
(John Godfrey Saxe, "The Blind Men and the Elephant.")

In the critique of *The Closing Circle* by Paul R. Ehrlich and John P. Holdren, and in the response by Barry Commoner (*Bulletin*, May 1972), each emphasizes especially one element of the problems we humans face on this Earth. The criticism seems unfair: Commoner in his response shows that what he actually said in his book differs materially from what Ehrlich and Holdren criticize, although they are correct in saying that we must grapple also with problems of over-population, excessive affluence and misdirected technology. But instead of quarreling about who is right and who is wrong, they should join with others in viewing the total situation, helping to take steps to mitigate the difficulties. Each has advanced consideration of his aspects of the situation. At the same time, both may be criticized for their rather elementary presentation of statistics, and analyses shown to favor their own cause. Percentages are especially hazardous, if not treacherous, in presenting information. For example, the first plastic container increased production an infinite per cent: the next hundred increased production by 10,000 per cent. I recall the announcement of a certain country that it was increasing payments to the disabled and elderly by 100 per cent; the actual increase was from about 6 cents to 12 cents per month.

We must, however, recognize that the Earth is finite, its resources limited: air, water, minerals and metals and other solid-Earth resources, the agricultural soils, and living space itself. We do not yet understand how all of these operate and interrelate. The books and other writings of these authors have done much to arouse interest and concern about these matters. Scientists and technologists should unite in meeting these problems on a broad front.

With regard to population and living space, we look on California as overcrowded, and word is that now more people are moving out than moving in. It has about 20 million people. Yet Japan, similar in size, arable land and other natural resources, has five times as many people. Though the Japanese are facing sharply increasing pollution problems, they live more in harmony with their landscape than do the Californians. And the United States as a whole almost seems to be underpopulated in comparison. So doubling the population of the United States does not seem to offer insuperable problems if we can learn to live in harmony with our surroundings.

With regard to air pollution, note that when a polar air mass invades a region, it quickly sweeps the pollution away, and a fresh, good atmosphere is restored. Away? We do not understand the natural processes in full; but through diffusion, turbulent mixing, chemical and other interactions, the restoration is quite complete. The carbon dioxide content of the atmosphere has increased a few hundredths of a per cent over recent years, as measured at a mountain peak observatory in Hawaii. This increase seems to offer no serious obstacle to life on Earth. The heavy metals, most prominently lead and mercury, are apparently no more persistent in the atmosphere than carbon monoxide, the sulphur compounds, the hydrocarbons, and other pollutants. If all of man's fires were extinguished tonight, the Earth's atmosphere would approach "pristine" condition in 10 days to 2 weeks. The increased background of carbon dioxide and perhaps of radiation and the particulates would linger. As the late W. T. Pecora, director of the U.S. Geological Survey, noted, three tremendous volcanic explosions poured more particulates and more noxious gases into the atmosphere than all of man's activities since the beginning of time (*Bulletin*, Oct. 1970). Thus, it seems that the air has tremendous recovery potentials. It has taken billions of years to evolve to its present state.

Water pollution offers greater difficulties. Yet if man were to stop pollution completely, a "pristine" condition would develop in most streams in about 5 to 10 years. The mud flats, the natural stream erosion—aggradation, degradation, and sedimentation (added to by the sediments washed from man-made scars left by mining and similar operations)—would continue.

"Pristine" does not necessarily mean, however, the sparkling clear water that the mind usually visualizes. In lakes, man-made pollution would be stabilized, in two or three decades, and then the lakes would continue their normal eutrophication toward extinction. Even Lake Erie would make great recovery, but the more polluted lakes might take up to a hundred years. The oceans, including beaches and estuaries, would make remarkable recovery in two or three decades.

On the land areas, soil erosion resulting from excavations and the destruction of cover would continue until the cover is reestablished, which in some cases may take several decades or even centuries. As shown by the work in the Tennessee Valley and elsewhere, reforestation and agricul-

tural practices can greatly accelerate the recovery process. If we accept the idea that before the Romans, the spine of Italy was heavily forested—and similarly the upland areas of Greece and Asia Minor, and Asia below the timber line—the natural restitution of the original forest condition might take several millennia.

The gross pollution of land areas by trash and junk would undergo substantial disintegration in humid areas in a 100 years, but mounds would remain as monuments to our folly for several centuries. In dry areas, the process would take longer, except as the junk heaps are buried in drifting sands. The archeologists should find interesting diggings. Highways, large open-pit mines, and large concrete dams would be in evidence until the downwarping of continental plates over millions of years returned these areas to the Earth's mantle.

But man's activities on Earth are not to be terminated tonight—or in a 100 years—and pollution will not be terminated. Thus, we must mitigate its effects. Man has the know-how, or the intelligence to secure the know-how, to accomplish mitigation and make the Earth habitable indefinitely. But does he have the will? It will cost a lot and take much effort just to get caught up, and then to continue to control the difficulties that our civilization has brought on. What science and technology have brought about, given the where-with-all and the continuing popular desire, science and technology can mitigate. Solutions for many of the problems are now in hand, ready for application on a large scale. Other techniques are in the laboratory stage, and still others are under research. Perhaps in some ways, the heavy-element contamination of our waters is the most difficult problem. For example, the mercury now in river and lake sediments will continue to contaminate those waters for a thousand years or more, unless we find some way to remove or stabilize the mercury. DDT, polychlorobiphenyl (PCB), and other compounds that are non- or very slowly degradable seem difficult. But beyond the essential research and development, the problems are economic, social and political. How will those problems be solved? It will take outstanding scientific, political and technological leadership.

The systems approach is essential. Pollution relates chiefly to renewable resources. The nonrenewable resource problems are passed over lightly both by Commoner, and Ehrlich and Holdren (and by Ehrlich in his book, *The Population Bomb*). Meadows et al. in *The Limits to Growth* take a systems approach, but are grossly in error in their assumptions.

As one looks back on the history of past civilizations, especially those of the Middle East and westward to Greece and Rome, he finds that many rose to great heights, then declined and ultimately disappeared. Historians usually conclude that the decline in most cases was chiefly the result of external influences, especially aggression. But there seems to be much evidence that, in many cases, the decline was largely caused by the *then usable* resources being used up. When a neighboring state developed a new resource (or technique), it became ascendant. The Greek city-states, pretty much in balance for many years, could not reach out very far without impinging on another city-state. The Romans developed a great but slow transportation system. Many influences led to its decline: malaria in the Pontine Marshes, corruption in high places, waters poisoned by lead pipe which the Romans invented, and the aggression of the "barbarians" from the north. But here, too, the laboriousness of transportation took its toll; the Romans had largely used up the usable resources within their reach.

Now the whole Earth is within one great transportation network. No point is now more than 48 hours away by direct air flight. Oil tankers carry hundreds of thousands of tons of petroleum over thousands of miles, aided by pipelines hundreds of miles long. The resource problem is now worldwide rather than local or regional. The resources are finite. The energy resource is thought by many to be the most critical. There is evidence that petroleum yields in the United States are permanently on the decline, and that by the year 2000 the world yield will have passed its peak, with natural gas not far behind. Coal will last for another 200 or 300 years; but as used now, the use of coal will increase air pollution. Strip mining destroys the land and increases erosion and tends to increase mine seepage. But these are problems that we can meet—if we will.

Atomic energy in great magnitude is within our reach; present type of reactors can meet our energy needs without serious damage to our environment if planned correctly, according to Alvin M. Weinberg and R. Philip Hammond ("Global Effects of Increased Use of Energy," *Bulletin*, March 1972). Later the breeder reactor and, still later, the fusion reactor will come; these should meet man's energy needs for 10,000 years or more. But 10,000 years make an interval only a little greater than the span of man's recorded history, and not more than a minute or two on the clock of man's existence on this planet. Surely within that interval solar energy will be developed, which has the potential of meeting man's needs for the duration of the solar system.

But what about other resources, especially the nonrenewable? The total supply of most metals is sharply limited; even now we must dig deeper, go farther, and use lower grade ores. No optimism is justified here. The supply can be extended substantially by intelligent recycling, which should be an important by-product of our cleaning up to maintain a clean environment. In the last hundred years or so, man has shown great ingenuity in the development of materials from resources hitherto of little use. The steel age is only a little over a hundred years old. Reinforced concrete is a product of this century. Aluminum was a rare metal at the

time of the World's Fair in 1906. Fiberglass, plastics, atomic energy, and the laser are developments, if not discoveries, that have come into use since World War II. These and similar products underscore what man can do, and offer hope for the future. Who knows what a shovelful of earth or a boulder holds for man a hundred or a thousand years hence?

We must not forget the great heritage that we received from the past—in mathematics, physics, art, literature, music, philosophy—as well as a largely unspoiled land. Is our heritage for the future to be an utterly despoiled and sterile land? We should surely hope better for our progeny. We must come back to the concept of a finite Earth. What is its ultimate capacity—not just for a century or even a millennium? Populationwise it has already exceeded its capacity were man entirely nomadic or even agrarian, although some may take China and India as exceptions to the latter. Japan, highly industrialized with its 105 million people, still has large green areas; and its large northern island, Hokkaido, about the size of Ohio, has only five million people. There is still room for population increase there, if they meet the increasing industrial and domestic pollution problems.

Population growth projections, even by the most skilled demographers are likely to involve gross error. I recall a widely publicized projection back in the days of the depression, in the mid-1930s. The population was then about 140 million; it was projected to increase slowly to reach 160 million by the year 2000, and then begin to decline slowly. Every long-range forecast needs to have its assumptions clearly qualified and stated—and even then, not taken seriously.

Waldo E. Smith is executive secretary emeritus of the American Geophysical Union.

A Note on Carbon Monoxide

Nature is responsible for about 90 per cent of all the carbon monoxide (CO) released into the atmosphere, according to Robert Robbins of Stanford Research Institute's Atmospheric Sciences Laboratory.

Although man's automobiles and factories each year spew out about 400 million tons of this poison gas, this is only about 10 per cent of the more than 3 billion tons that atmospheric scientists now believe are produced every year by natural sources.

From 1967-1970, under a project supported by the U.S. Public Health Service and the National Science Foundation, SRI researchers Robbins and Elmer Robinson measured the atmospheric concentration of CO in remote ocean and arctic regions and elsewhere. They calculated that about 600 million tons of CO are normally present in the global atmosphere. This figure agreed reasonably well with earlier estimates based on very limited data.

From this figure for global concentration and from the known emissions of CO into the atmosphere—mostly by man-made sources—the SRI researchers then calculated the average lifetime of CO in the atmosphere to be about 2 years.

In 1969, Bernard Weinstock of Ford Motor Co. calculated the lifetime in the atmosphere of CO containing a radioactive isotope of carbon. Since the chemical and physical behavior of CO containing this isotope is identical to that of normal CO, he concluded that the lifetimes of the two types should be the same. The lifetime that he obtained by this method was only about a tenth of a year.

Scientists reasoned that if the actual lifetime of CO is this brief—and Weinstock's calculation has since been confirmed—then there must be some very large, heretofore unknown source to account for the measured concentration of this gas.

They began to suspect that methane might be this source. Robbins and Robinson had previously estimated that 1.6 billion tons of this "marsh gas" are released into the atmosphere every year, principally from decaying vegetation in marshlands.

All of this methane is now believed to be converted eventually to CO, producing 3 billion tons or more of this gas. The principal agent of this conversion is believed to be an exotic oxidizer known as the hydroxyl radical, which is present in the atmosphere in very small amounts.

Other much smaller natural sources include biological materials in the ocean, decaying chlorophyll, and certain growing plants.

Despite the enormous amounts of CO released into the atmosphere every year, the atmospheric concentration of this gas has remained fairly constant for many years at about 0.1 part of CO per million parts of other gases. Robbins and his associates have found that the air trapped in ice samples dating from 600 B.C. to 1850 A.D. has about the same concentration of CO as the measured global concentration today.

The principal sink for all this CO is believed to be the same hydroxyl radical that produces this gas from methane in the first place. This material oxidizes CO to carbon dioxide (CO_2).

Microorganisms in the soil are another important sink for CO. An SRI group led by Robert Inman has found that most soils contain fungi that evidently convert CO to CO_2. They estimate that, under favorable conditions, the soil surface of the United States alone is capable of removing about 600 million tons of CO from the atmosphere per year—compared with the 400 million tons produced annually by man.

Even if man's CO emissions were to increase 10 to 100 times, they would probably have no serious effect on the atmospheric background concentration of CO.

But this would be small consolation for the man with his nose in an exhaust pipe. Such a man would be breathing at least 10,000 parts per million of CO—enough to kill him within 5 minutes even if it contained no other pollutants. A concentration of 100 ppm could give him a splitting headache after a few hours' exposure.

Luckily, 100 ppm is an unusually high concentration of CO. On the freeway during the rush hour the air is typically around 15 ppm. It rarely gets up as high as 100 ppm even in a freeway traffic jam.

Demophora

Greeks Had Words for the Interdependence of Biological and Technical Growth

(*Biological Conservation*, October 1972)

The purpose of this note is to publicize a new and potentially useful word introduced at the International Conference on Environmental Future held in Finland, June 27 to July 3, 1971. The definition and context of use appears in a paper by J. R. Vallentyne in the conference proceedings (N. Polunin, ed., "The Environmental Future," 1972).

The word is *demophora* as a noun, *demophoric* as an adjective, with suggested accents on the second and third syllables, respectively, as in geography and geographic. Derived from Greek (*demos*, population; *phora*, production), it is intended for use in combined reference to human population and technological production-consumption, using the latter term to encompass not only products of manufacture and cultivation, but also the resources consumed and wastes generated in their production and use. The word stresses the unity and interdependence of biological and technological phenomena inherent in the "man-machine" concept, with implications that are primarily, though not exclusively, metabolic.

Two considerations motivated us to invent the word. The first was a scientific need to express the interplay of cause and effect relationships between human population phenomena and technological growth. The second was a need for new tactics and strategies to facilitate global agreement to control the rates of human population growth, environmental pollution, and resource depletion.

The scientific value of the word hinges on the existence of causal interrelationships between population and technological phenomena. The surprising thing is not the lack of evidence for such relationships, but the absence of a word to describe and epitomize them. Numerous causal connections between population and technological phenomena were, for example, described by Adam Smith in *The Wealth of Nations*, and others uncovered since that time now form much of the basis of current socioeconomic doctrine.

In a similar sense, it is widely accepted by human evolutionists that tool-using primates and their associated "technologies" evolved as coupled systems—a change in one inducing a change in the other—with selection operating on the total system as a unit. Also, from an ecological point of view, R. Margalef has pointed out that the biomass and metabolism of Homo sapiens must logically be defined to include tools, machines, factories, vehicles, and the like, in addition to biological tissues. Finally, the combination of the two aspects of human "metabolism" into a single word permits certain characteristics of the "man-machine system," such as momentum, to be described in a manner that cannot adequately be conveyed by the same components individually. For the above reasons we feel that *Demophoric Explosion* and *Demophoric Control* will ultimately be accepted as more accurate and meaningful terms than any or all of Population Explosion, Population Control, Technological Revolution, and the like.

The suggestion that the word may be useful in an "Orwellian" sense is based on a belief in the urgent need for global agreements to control the explosive growth of human population and technological production-consumption. Many authorities are convinced that if these matters are not brought under human control during the present decade, then a number of unpredictable, highly disruptive and perhaps irreversible, changes will result in subsequent decades. The problem at the present time is the low probability of agreement on such controls unless viewpoints change. Neither individuals nor nations look with favor on the imposition of external controls.

It is possible, however, to put forward an opposite view—that most individuals and governments favor global demophoric controls. Technologically developed nations, for example, appear to favor instituting global controls over the growth of human population—particularly in areas where additional population growth is likely to bring reduced socioeconomic returns (e.g., over-populated, underdeveloped nations and densely populated urban centers in all parts of the world). Likewise, developing nations appear to be overwhelmingly in favor of global measures for the control of technological production-consumption—particularly in luxury-glutted, developed nations where current levels of technological production-consumption are high and still rising. Viewed in this manner one could conclude, with some sense of reality, that a barterable basis for agreement does exist.

The combination of the two phenomena into a single, scientifically-based word could be a critical factor in facilitating discussion and agreement on measures for the control of human population and technological production-consumption (demophoric control). The mere presence of the word could force all parties to recognize opportunities for individual gain through compromise in a context where, by definition, agreement could not be miscontrued as a "loss of face." Once such discussions have been initiated, the critical questions will hinge on specific control mechanisms and the nature of "trade-off" relationships between growth of human population and technological production-consumption. If the word facilitates discussion on any of these matters, it will have served a useful and timely purpose; if not, little will have been lost.

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SALLY JACOBSEN

This is the sixth in a series of Bulletin *interviews with scientists engaged in several fields of research on the nature and implications of their work. The interviews are designed to expand communication between scientific researchers and the public. This month we invited Eugene N. Parker to discuss his work on the solar wind, magnetic fields and cosmic rays. Dr. Parker predicted the existence of the solar wind in 1958. He is professor and chairman of the department of astronomy and astrophysics at the University of Chicago.*

Research Review

Eugene Parker

On the Solar Wind, Magnetic Fields and Earth Weather

It has been approximately a decade since the existence of the solar wind was verified by space probes. What are some of the important questions about outer space that researchers are presently trying to answer?

I would say that some of the fundamental questions we want to get at are the behavior of Jupiter and the changing intensity of cosmic rays farther out in the solar system. These are related to another question: What is the behavior of the solar wind near Jupiter and beyond? Both the Pioneer 10 mission, which is now heading for Jupiter, and a future Mariner-Jupiter-Saturn mission may give us some answers to these questions. (Pioneer 10, launched March 2, 1972, will swing by Jupiter and, accelerated by the gravitational field of Jupiter, escape from the solar system.)

We are also puzzled about the fluid motion of the interior of the Sun and the production of fast particles by the Sun. The origin of solar flares and magnetic field annihilation is another fundamental problem that we have been trying to understand for 10 years. We know more now than we did, but we still cannot say exactly how field annihilation happens. I'm afraid that it's a complicated process in which there are a number of things going on simultaneously. Another puzzle is the effect of the solar wind on global weather. Briefly, that's the way the frontier looks for research in this area.

Has any information relating to these questions been received from the Pioneer 10 spacecraft?

We've received data concerning the intensity of cosmic rays as the spacecraft moves toward Jupiter. The theory is that cosmic ray intensity should increase as the spacecraft goes farther out in the solar system. The solar wind tends to sweep out the cosmic rays from the inner solar system, so we expect the cosmic rays to increase on the way to Jupiter. We thought that most of the modulation of the cosmic rays takes place between here and Jupiter, which means that by the time Pioneer 10 gets to Jupiter the instruments should be recording twice the cosmic ray intensity plus a whole flock of low energy ones that never get in to Earth. Although Pioneer is about three-quarters of the way to Jupiter, this rise in intensity has not occurred. There's some rise, but not as much as was expected. This means that the effect on cosmic rays takes place farther out in the solar system. Perhaps this will come out in the Mariner-Jupiter-Saturn flight being planned.

Another interesting observation is that there isn't all the sand- and gravel-sized debris in the asteroid belt that was expected. There were some predictions that the spacecraft would never survive going through the asteroid belt. This is a puzzling development because it is believed that most of the small debris is produced by the collision and

crushing of asteroids. If that is the case, why is there not more debris? All this information is so new that people have not really had time to digest it yet.

What are some of the curiosities of Jupiter that we may learn about from the flybys?

The red spot on Jupiter is an enigma. There's some evidence that it appeared about 200 or 300 years ago. Some of the very earliest observations of Jupiter, made with fairly sophisticated instruments, failed to detect the red spot. The red spot is probably a weather pattern. At first, some people thought it was an iceberg floating in the atmosphere, but the atmosphere is hydrogen and nothing floats in hydrogen. Now we think it may be a circulation pattern. It has been suggested that it may represent a circulation pattern over a high piece of ground or a hole in the ground. Either condition would trap the circulation pattern.

The radiation belts on Jupiter are another puzzle. Jupiter has a very intense magnetic field, which we know from the radio emissions from the planet. Radio astronomers found that it apparently has a dipole field like the Earth's, but it must be 20 times stronger. There are particles trapped in it, but only the trapped electrons give off radio emissions. How many protons are there? It was an academic question until people began thinking about sending a spacecraft there. Then they began to worry about the damage that fast protons might do to a spacecraft.

A study group headed by James A. Van Allen, professor of physics at the University of Iowa, found that Jupiter may have a fantastically intense radiation belt of protons that would wipe out any vehicle coming within six Jupiter radii or 250,000 miles of the planet. In fact, the only thing that they could think of that would cut down on these fantastic fluxes of fast particles was the possibility that Io, Jupiter's satellite, might absorb or stop the particles. So the question is whether a spacecraft can get passed Io without being damaged by fast protons.

The Wind from the Sun

Let's digress a moment and talk about the solar wind. How did you come to call it a wind?

It was deliberately called a wind because that's a fairly accurate description of it. It is simply a rapid flow of gas outward from the Sun through the solar system. The velocity of the wind is approximately a million miles an hour.

It is a plasma, meaning an ionized gas. The gases we normally encounter are not ionized, but due to the high temperature of the place where it originates the electrons are stripped off the atoms, giving ions and electrons.

The wind carries a million tons of gas per second away from the Sun, which sounds awfully impressive. But in the five billion year life of the Sun, it would carry away about 1/100 of one per cent of the mass of the Sun. From the Sun's point of view, it is completely trivial. So the solar wind probably contributes very little to the aging of the Sun at the present time, but there are some reasons to believe that when the Sun was much younger it had a much more vigorous solar wind. It could conceivably have been important to the Sun then, but now it's important only to the planets.

The solar wind is caused by the outer atmosphere of the Sun, the corona, which is a gas with a temperature of a couple of million degrees. It is so hot that it simply expands outward into space. Although the Sun has a strong gravitational field, the two million degree temperature of the gas is enough to make it stream outward. It streams outward all the time and in all directions from the Sun.

The Sun is largely hydrogen with about 10 per cent helium. Interestingly enough, the solar wind is depleted in helium. Astronomers have made measurements of the amount of helium in the Sun, and they usually get a figure of one atom in 10 or 15. Helium is about four times as massive as hydrogen. The explanation is that helium is a heavier atom, and it settles down through the corona. The corona is expanding upward fast enough to carry out some of the helium atoms, but only some.

When a solar flare occurs, the helium in the solar wind may increase to 30 per cent. The explanation is that the helium has been settling out of the corona and accumulating like a layer of smog at the bottom of the corona. When a solar flare occurs, everything goes up, resulting in the extra helium in the solar wind.

Comet Tails Clue

How was the solar wind discovered?

The first clue to its existence came from comet tails. The German astrophysicist Ludwig Franz Biermann about 1950 got interested in comet tails. Elementary astronomy books explain that the comet tail points away from the Sun because of radiation pressure from the Sun. The light shining on the tail pushes it away.

By the 1950s calculations could be made of the cross-sections to determine how the comet tail really interacted with the light from the Sun. Biermann pointed out that there was nowhere near enough push to make comet tails behave the way they did. Not only do they point away from the Sun, but they also have little knots that are accelerated at very high rates outward from the comet. Biermann thought about the possibilities and pointed out that the only explanation for this rapid blowing of comet tails was what was called solar corpuscular radiation. Previously, it was suggested that once in a while the Sun emitted large amounts of gas (solar corpuscular radiation)

"I've been talking about this phenomenon in the last year because I think it is important. We will never forecast the weather until the meteorologist learns to take account of such big effects as these."

at a high speed, but Biermann observed that the comet tails showed that there was always material streaming out of the Sun. And it was the same no matter where the comet came from around the Sun.

I talked to him in 1957. The more I talked to him, the more I began to feel that we were fenced in. There couldn't be anything wrong with his observations, which gave a completely different picture than the conventional one.

I also talked to the late Sydney Chapman, who was a geophysicist at the University of Colorado, about the solar corona. He pointed out that the corona is so hot that it should extend clear to the orbit of the Earth. The more I thought about that remark and Biermann's remark, the more I came to the conclusion that the corona and solar corpuscular radiation must be the same thing.

I wrote down the hydrodynamic equation for an extended atmosphere, and there it was: The gravity of the Sun isn't enough to hold it in, and we have a solar wind. It is just the ordinary hydrodynamic equation.

The observational estimates of the velocity made in those days were a bit high because they were based on the sudden outbursts at the time of a flare when there is extra heating. They were about 1,000 to 2,000 kilometers per second. A good figure for the quiet day solar wind is 300 to 400 kilometers per second (250 miles per second).

I called it the solar wind because I felt that solar corpuscular radiation gives the wrong idea. With that term, one thinks of individual particles being shot out, which was the original picture we had. But it really is an ordinary flow of gas.

In 1959, the Soviet moon probe, Lunik I, detected the solar wind. It was a very crude measurement; it merely said that there's hydrogen streaming faster than 60 kilometers per second. The instrument was not designed to detect the solar wind; it merely gave this lower limit of velocity.

Then the Mariner II satellite went up in 1962 en route to Venus. The instruments, prepared by the Jet Propulsion Laboratory, were sent up with the idea that there was a solar wind despite all the scepticism. They detected a solar wind with a velocity of 300 to 400 kilometers per second and a density of five atoms per cubic centimeter. (Lunik I and Mariner II were the earliest vehicles to travel beyond the Earth's magnetosphere, which keeps out the solar wind.)

People have studied the solar wind at great length ever since. It has all kinds of interesting internal structures. It is always turbulent, and it has funny rotational discontinuities. The magnetic field will be in one direction for hours, and all of a sudden will shift 90° or 180° from what it was. This all happens in just a very few seconds. It is a strange phenomenon. I don't know if it has any importance.

What type of instruments are used to detect the solar wind?

To detect the solar wind, a grid of negatively charged wires is set up to repel all the electrons from the solar wind so that only the ions flow through. The ions are collected, that is, the charges pile up on an electrode, and the current is measured. This grid does not tell how fast the wind is flying, which is the trouble the Soviets had. They knew something was there, but they couldn't tell how fast it was going.

To do that, a positive grid is placed behind the negative one. The voltage is raised until the inflow of ions is shut off. The velocity of the incoming ions can be computed from the volts needed to shut off the current. There are some problems, such as the Sun shines in and produces photoelectrons, creating false currents.

Beyond the Planets

How far does the wind extend out in space?

Nobody knows. It spreads out as it goes out. Eventually it gets so spread out and tenuous that it must be stopped by interstellar gas, which is between the stars. An estimate of the distance at which the wind is stopped by the interstellar gas would be somewhere between 50 and 200 astronomical units, an astronomical unit being the distance between the Earth and Sun. Pluto is at 50. The wind might not get any farther than Pluto, but then again it might go several times farther.

What is the effect of the solar wind on the cosmic rays between the Earth and the Sun?

There is probably relatively little effect between here and the Sun because there is not enough turbulence in the wind. This will be measured in the planned Mercury-Venus flyby.

The interesting thing between here and the Sun are the solar cosmic rays, the high velocity particles produced by the Sun. They come blasting out from the Sun and are affected by the magnetic fields.

Galactic cosmic rays come from such things as supernovae, pulsars and who knows what else. They are simply fast particles that seem to penetrate everywhere in space. They are largely the nuclei of atoms accelerated to nearly the velocity of light. They consist not only of hydrogen, but also of the nuclei of heavier atoms. They are really

a sample of material from whatever it is that produces them. If they are from supernovae, then they are a sample of material from supernovae accelerated to the speed of light.

John A. Simpson, professor of physics, and Peter Meyer, professor of physics, here at the University of Chicago have been interested in investigating exactly which nuclei are present. By looking at the various isotopes, scientists can tell the temperature and density in the object from which the cosmic rays come.

Cosmic rays exert about the same pressure as starlight, about 10^{-12} dynes per square centimeter, which is a big pressure in the galaxy. Cosmic rays are one of the things that inflates the disc of the galaxy. If cosmic rays were not present, the disc would be half as thick as it really is.

Cosmic rays have been with us a billion years. We know that because they produce radioactive nuclei in meteorites. The radioisotopes would not be there if they had not been produced by cosmic rays because radioactive elements originally present in the meteorite would have long since decayed.

Sometimes there's speculation that cosmic rays are responsible for most genetic changes. There's even speculation that the dinosaurs were wiped out by an excess of cosmic rays. It would be fascinating if there were some connection, but careful examination shows that there's no justification for the idea.

What is the relationship between the magnetic fields and the solar wind?

The Sun generates the magnetic fields. The solar wind is responsible for stretching out the magnetic fields of the Sun. It carries the fields with it and stretches out the lines of force from the Sun. Interplanetary space is full of weak magnetic fields, all pulled out from the Sun.

The magnetosphere of the Earth is the region enclosed by the magnetic field of the Earth. The solar wind blows against the magnetic field of the Earth but doesn't penetrate it. It pushes the Earth's field down to a closed, finite body on the sunward side where it is hit directly by the wind. The field extends only 10 times the radius of the Earth, or 40,000 miles, on the sunward side. On the back side, where there is no wind, the magnetic field streams out in a long cylindrical volume 150,000 miles in diameter. So the magnetic field is not really a sphere, even though it's called the magnetosphere, but a cylinder.

The agitation of the magnetosphere by the solar wind, which is always shifting and shaking as well as turning over, folds particles into the Earth's magnetic field and gives them very high velocities. This creates the Van Allen belts, which were named after James Van Allen.

The shaking of the magnetic field and folding of particles into the field are responsible for the auroras. The auroras and radiation belts are produced by the solar wind. If the wind were shut off, these would soon decay away. This may have happened three centuries ago. If we look back at the records of sunspots, which have been kept very diligently since Galileo started using the telescope, the number of sunspots during 1645 to 1715 was extremely low, just a fraction of what we normally see. Normally, the sunspot cycle is a 22-year period; every 11 years there is a half cycle and a new batch of sunspots. But during this period, the 11-year or 22-year cycle could still be seen, but the sunspots tended to appear only at the peak of the cycle instead of throughout. Instead of thousands of sunspots, there were only hundreds, and they occurred only in one hemisphere of the Sun.

Also, during that time the big auroral events, which are commonplace in the Scandinavian countries, practically disappeared. In addition, during that period people did not observe the solar corona during an eclipse. The corona is conspicuous to the naked eye during a total eclipse. Lack of a corona means that there was no heating of the outer atmosphere of the Sun. Therefore, perhaps there was no solar wind, which would explain the lack of aurora. It might have been turned on once in a while, but it looks as though most of the time it was turned off. There are no weather records to consult. Certainly there was nothing catastrophic for life here on Earth, but it may well be that the weather patterns changed during that time. Who knows? This phenomenon has not happened since. Nobody can explain what in fact did happen. Why should the Sun continue the cycle, yet turn the whole thing so far down that the effects could hardly be seen? I don't know how to explore that, but it shows that there are many curious things going on in our solar system that would be nice to know about.

Would you describe solar flares? Are they related to sunspots?

Solar flares and sunspots are associated. A sunspot is a relatively cool region on the Sun. The surface of the Sun is normally 6,000° Kelvin, and the sunspot is only 4,500° Kelvin. In the last century people suggested that there are intelligent beings living in sunspots because they are cool, but 4,500° is still pretty hot.

A sunspot is a place where a very strong magnetic field comes up to the surface of the Sun. A typical sunspot field is 3,000 gauss, about 6,000 times as strong as the field of Earth. The field is associated with keeping the spot cool, and the coolness seems to be associated with making the field strong because the gas collapses and the field is pushed in. Sunspots are not yet entirely understood. They represent a place where a strong magnetic field comes through the surface of the Sun.

Whenever the field pattern gets very compli-

"Weather patterns . . . sometimes involve the mixing of north-south winds that come down from Canada and up from the Gulf of Mexico. The result is the wild temperature variations that we are experiencing this winter . . . . The question is: What periodically stirs up the storms from Canada and the Gulf?"

cated, a flare is likely to develop. A flare can be seen in visible light, and it also shows up in the spectra of radio signals, ultraviolet light and x-rays. The flare is a signal that a large amount of energy has been released very abruptly. A flare may last 10, 20 or 30 minutes. It can be compared to a small explosion, but it is a very complicated type of explosion. That's why we still don't know what a flare really is.

During a flare particles are accelerated at high speeds and fill space with fluxes of fast particles and very intense radiation. These outbursts from the Sun are something like the storms on the surface of the Earth. About every 50 years we get a fantastically big one. There was concern in the space program that a man might be on his way to the Moon and be hit with a big burst of radiation from the Sun. The spacecraft was designed so that a man inside could survive most of the big storms. But a man exposed on the surface of the moon in the landing module would not have sufficient protection if a particularly large flare occurred. A very intricate warning system was devised so that if a flare did occur, an astronaut would be able to return to the command module or go underground for protection. But it turned out that—the Sun being capricious as always—this sunspot cycle was very anemic. There were several big flares prior to putting a man in space, but apart from last August there hasn't been a respectable flare in a long time. In 1956, there was a big one that caused the counters at ground level to soar way up with the extra radiation.

All of this activity can be traced back to the generation of magnetic fields on the Sun. At the present time, it seems as though those magnetic fields are generated by the fluid motions of the Sun. The gases of which the Sun is composed are continually churning and flowing in ways that are similar to the ocean currents on Earth.

Because the Sun rotates, the activity tends to be cyclonic. The gases generate magnetic fields, which in turn generate exotic effects, such as flares and fast particles, and contribute to the heating of the solar corona and the solar wind.

The processes on the Sun present a very complicated puzzle. In fact, it is sufficiently complicated that we may never come up with a concise, simple answer that incorporates all the important details. I am fond of drawing a comparison between the Sun's processes and terrestrial weather, which is also terribly complicated. Yes, we understand a great deal about the weather; but no, we cannot predict the weather for tomorrow or next week. We can understand solar events up to a certain point so that after one occurs we can describe more or less what happened. But we have not gotten to the point of having a concise, complete quantitative theory. The solar flare is particularly elusive in this respect.

Solar Wind and Earth Weather

What effect does the solar wind have on global weather?

Let me begin by explaining that the weather in the United States and Canada comes from three sources: the Gulf of Mexico, the Pacific Coast and the Aleutian Islands. The weather patterns in the United States and elsewhere in the world sometimes involve the mixing of north-south winds that come down from Canada and up from the Gulf of Mexico. The result is the wild temperature variations that we are experiencing this winter. On the other hand, sometimes the weather patterns settle down with strong east-west winds. When this happens, jet airplanes have strong tail winds and break records flying from Los Angeles to New York. During the strong east-west wind pattern, there is not much Arctic or Gulf air.

The question is: What periodically stirs up the storms from Canada and the Gulf? It now seems clear that many of the storms that come sweeping down from Canada originate over the Aleutian Islands. A storm starts when a fold or trough appears in an isobaric surface. This trough is the beginning of a storm. It deepens and contorts to become a storm. It then comes across Canada and may sweep down across the States, too.

About 15 years ago, a number of people, including N.J. MacDonald, atmospheric physicist, Air Force Cambridge Research Laboratories, Bedford, Mass., D.D. Woodbridge, physicist, Florida Institute of Technology, and Walter Orr Roberts, president of the University Corporation for Atmospheric Research, found a very curious thing. A trough is most likely to form within a few days of a magnetic storm or aurora. A magnetic storm, the shaking of the Earth's magnetic field, occurs when a big blast from the Sun hits the Earth. They found that if there is a magnetic storm and an aurora, it is fairly certain that two, three or four days later a trough will form. The troughs that form after a magnetic storm are bigger and stronger than troughs formed in the absence of a magnetic storm. Most of our big storms occur after a magnetic storm.

But there has been relatively little interest in this even though the statistics show that it is a big effect. Indeed, this idea has been rejected outright in many quarters. We don't know what the connection between magnetic and terrestrial storms is, but I think that Roberts has taken a very rational view of this. He says there are many possible explanations. For instance, suppose that

"Auroras and magnetic storms as well as their effects on the upper and lower atmosphere of the Earth can be studied only with instruments carried out through the atmosphere into space. We can theorize, but without space probes we will never know the answers."

an aurora is a flux of fast particles that hits the top of the atmosphere and excites the atoms, which creates the auroral glow. In the process, ions are produced. If these ions are mixed into the atmosphere—about 40,000 to 50,000 feet up—by winds, they would form the nuclei for the condensation of moisture, particularly in high cirrus clouds. The greenhouse effect will occur: The cirrus clouds will let light from the Sun through, but not let the infrared light back through. The temperature begins to rise, and a trough forms. This is one explanation of how the mechanism might work.

I've been talking about this phenomenon in the last year because I think it is important. We will never forecast the weather until the meteorologist learns to take account of such big effects as these.

Research and Relevancy

I think this is an excellent example of research with no obvious practical value breaking new ground and discovering some very important things. In fact, if we are breaking new ground, we can never anticipate or justify what we are doing in terms of practical application. If we knew ahead of time what the effects would be, we would not be breaking new ground.

In the past few years, funds for basic research have been drastically cut. Why do you think this has happened?

It's fashionable. We seem to vacillate between total indifference to science on the one hand, interspersed with flights of egotism and hysteria. If we look back at the history of the U.S. attitude toward science, this would be obvious. Starting in the 1920s, we were more or less oblivious to science. We had a few people who did some very good work, mainly experimental; but there were hardly any theoreticians, except Josiah Willard Gibbs. We more or less slumbered.

Then fission was discovered in Germany in the 1930s, and we suddenly woke up. Fortunately, by that time, we had people like J. Robert Oppenheimer, who had developed a new school of theoretical physics. Even luckier, we had political refugees, some of whom were the best European scientists. By sheer luck, we were able to develop nuclear power and bombs, racing against what we thought was the German development at that time. Then the United States realized that it had to stay in the field of science. But soon complacency set in. Since we had made these big strides forward, it was very easy for those outside of science—and even those within—to say that now we can relax.

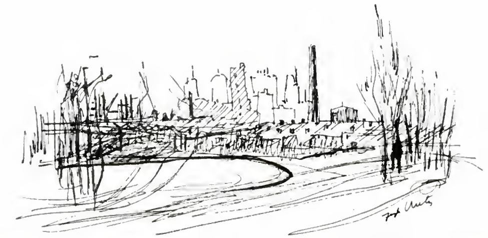
Then came the Soviet development—Sputnik. Also, the Soviets were the first to reach a certain stage in the development of the hydrogen bomb, which was a shock to the American people. Following Sputnik, there was simply hysteria. Scientific projects, good or bad, could be funded relatively easily. Everything was geared to science. Science was going to make us a world power. Now we are a world power, and that hasn't worked out as well as we had hoped. The reaction is that science is ridiculous and that it has caused us many problems. I don't think our problems were caused by science. We had the problems before, and we continue to have them. But now it's fashionable, particularly among intellectuals, to say, "We don't do science, we only do practical things."

The fashions are 99 per cent irrelevant. They always have been and probably always will be. I suspect that funds for science will be cut considerably more, and I suspect that our presently deteriorating position with regard to the development of new technical projects will continue to worsen. The only solutions that have been offered are short-sighted ones. For example, if our engineering falls behind, the short-sighted answer is to produce more engineers. This may be all right for a few years, but basic scientific discoveries are needed as a foundation for engineering.

I suspect that we will get into a crisis situation and the fashions will go the other way. There will be public hysteria, and then there will be a lot of money for projects. And the money will be used very inefficiently because it will be used for crash programs. Nothing is ever done until there is a crisis. That's why so many programs and projects have been so expensive.

Do you need the space probes for your research?

We can get some things from investigations on the Earth. But without spacecraft we would simply have to forget about some problems. For instance, the question of whether Jupiter and Saturn have radiation belts can only be explored by spacecraft. The properties of the solar wind in the outer solar system falls into the same category. The x-ray and ultraviolet studies of the Sun and solar flares can be studied only with instruments carried outside the atmosphere of Earth. Auroras and magnetic storms as well as their effects on the upper and lower atmosphere of the Earth can be studied only with instruments carried out through the atmosphere into space. We can theorize, but without space probes we will never know the answers.



Reflections on Politics and Pluralism: A Response To Jeremiads

ANDREW M. GREELEY

(Editor's Note: Not all sociopolitical analysis must be couched in the stiff, impersonal prose of the technical journal. An informal, subjective approach to the review of contemporary issues in the social sciences characterizes the widely published work of the Rev. Andrew M. Greeley, a Chicago sociologist. Fr. Greeley, a Roman Catholic priest, is director, Center for the Study of American Pluralism, National Opinion Research Center, University of Chicago.)

Out on the prairie soil of Illinois where I live I devote Saturday mornings to absorbing the flow of wisdom coming in my direction from the Olympian heights of editorial offices. I diligently pursue *Commentary, The Public Interest, Social Policy, New York Review of Books, Saturday Review, Society, Harper's, Atlantic, Commonweal, Christian Century, Intellectual Digest, The New Republic, Psychology Today,* and the previous Sunday's *New York Times*, which has just been delivered five days late by our socialist postal service. Having conscientiously discharged my duty, I turn for light escapist reading to St. John of the Cross's *Dark Night of the Soul.*

By noon every Saturday I am convinced that not only are things disastrously bad, indeed apocalyptically so, but that they are rapidly getting even worse. I am further discouraged by the thought that the authors and editors of these worthy journals are probably having a splendid time at cocktail parties in their weekend homes in suburbia, while their work has put a pall on my weekend that can be lifted only by an improbable victory of the Chicago Bears on Sunday afternoon.

Depending on which journal has made the strongest impact, I am faced with the thought that we are involved in an environmental *Gotterdammerung,* or that we live in a society of horrendous inequality and injustice, or that legions of hard-hat ethnic middle-American racists are closing in on all sides. All education has failed, all social policy has failed, everything has failed. We are in the grip of a generation gap, future shock, backlash, alienation, permissiveness, revolution, a heroin epidemic, and the collapse of the family. The American citizenry is turning away from its conscience and refusing to "come home"; the American people are evil, corrupt, immoral, fundamentally wrong. One-dimensional man dominates what is left of the environment; our country is failing to reorder its priorities; American society is coming apart.

There are a number of replies one could make to the prophets of doom. One could cite, for example, excellent research data that indicates that the family is not collapsing, that there is no such thing as either future shock or a generation gap, that there is relatively little alienation west of the Hudson River, that young people are not revolutionaries, that the amount of sexual promiscuity does not seem to have changed much in several decades, and that the ethnic backlash seems to be invisible.

Alternatively, one could raise questions, both logical and philosophical, about collective guilt. Is the whole society fundamentally immoral or just part of it? If so, what part? Eighty per cent? Forty per cent? Is the society so corrupt as to be irredeemable? If it is redeemable, what are the dimensions within it that make it so? Might not Hannah Arendt be correct after all when she said that if everyone is guilty then no one is?

Or one could try to engage in systematic discussion of the complexities and nuances of all our social problems and trot out vast quantities of technical data which would indicate that neither are the problems so bad nor the solutions so obvious as the wise men would have us believe.

But to engage in such dialectic efforts would be

a waste of time, for we are dealing here with neither issues about which men of good will can debate nor with questions of fact for which empirical evidence can offer answers. We are dealing with symbol systems, with implicit pictures that enable the sophisticated intellectual elites of a society to order and interpret the phenomena that impinge on their consciousnesses when they consider the society of which they are a part. These symbol systems are socially constructed and reinforced and become the basic categories of analysis and explanation. In the words of David Matza:

> Assumptions are usually implicit. They tend to remain beyond the reach of such intellectual correctives as argument, criticism, and scrutiny . . . . Left unattended they return to haunt us by shaping or building theories that purport to explain major social phenomena. Assumptions may prompt us to notice or to ignore discrepancies or patterns that may be observed in the empirical world. (David Matza, *Delinquency and Drift* (New York: John Wiley & Sons, 1964).)

We suffered recently a national election in which two upper middle-class groups fought for political power—Harvard Yard versus Orange County, the New Class versus Disney, or as Kevin Phillips, the author of *The Emerging Republican Majority*, remarked to me, PhDs versus those who eat lunch at Howard Johnson's. The New Class of intellectuals and social helpers, conscious of its own intellectual and moral excellence and the superiority of its critical analysis of American society and culture and its surpassing worthiness to govern, took control of the Democratic Party in 1972 and ran the most open, just, fair, and reformed political convention in the history of the land. This was a convention in which all the oppressed groups in the society were duly represented (with the proviso that the best representatives of the oppressed were those of high academic rank). That convention proceeded in the most open and democratic way to purify itself of the corrupt and immoral old politicians and the cigar-chomping labor bosses who had managed to get in. It then nominated a man whose intelligence was certified by his PhD and whose moral excellence could be questioned by no one. Faced with a choice between those who seemed to have principles for every occasion and those who had no principles at all, the American public decisively chose the latter. If the responses obtained by thousands of pollsters who milled around the country in early and middle autumn 1972 showed anything at all, it was that the sentiment felt by most Americans toward the very "clear choice" offered them was basically similar to the sentiment of that unfortunate Mercutio caught in the crossfire between the Montagues and Capulets, "A plague on both your houses!"

It is with this puzzle, I think, that any attempt to reappraise contemporary American society must begin. Virtually everyone in the intelligentsia—from *The Public Interest* and *Commentary* on the one hand to *Social Policy* and the *New York Review of Books* on the other, from Norman Podhoretz to Anthony Lewis—was absolutely convinced that this was a "clear choice" election. The vast majority of Americans seemed equally convinced that it was a "lesser of two evils" election. The best minds of the country were absolutely certain that this would be an election to divide the sheep from the goats and would clearly indicate whether the American people were ready to "come home again" or not.

I can only say that the best minds of the country were profoundly wrong, and it is hoped that some of them will bracket, temporarily at least, their pictures and their assumptions, their symbols and their myths, and consider the possibility that they may not have the foggiest notion of what the hell is going on out there beyond the Hudson River. To put the matter somewhat differently, when approximately three-fifths of the population supports the liberal position on such major issues as gun control, integration, guaranteed annual income, pollution, and peace, the liberal propensity for losing elections ought to raise in the minds of at least some of the members of the New Class the possibility that the problem may not be so much the immorality of the American public as their own myopia. As a young Polish-American historian puts it:

> During the first hundred years that the Polish community in Detroit has existed, the American view of the immigrant and his progeny has changed considerably. The brutish, antidemocratic ignoramus; the strikebreaking supplanter of honest American labor; the advance guard of anarchism and Bolshevism; the mindless tool of the papal conspiracy was transformed by the 1940s into the kindly, gentle, slightly comic fellow who, waving his citizenship papers proudly, burbled heartwarming patriotic cliches in his broken, night school English. Now, he is again transformed into the racist hard-hat. These racial stereotypes, many of them conflicting, reveal much more about the projected hopes and fears of American society than they do about the immigrant. (Thaddeus Radzialowski, "The View from the Polish Ghetto: Some Observations on the First Hundred Years in Detroit." Unpublished paper, Department of History, Southwestern Minnesota State College, Marshall, Minn., 1972.)

Telling It Like It Was

I am suggesting, therefore, that one of the items that should be on all our agendas for the next decade or so is an exploration of America—not the America we perceive through the smog banks lying low over the Jersey flats, nor the America of wandering journalists like Peter Schrag or Bill Moyers, for whom "telling it like it is" means telling it like they knew it was before they got to Newark Airport. As my own modest contribution toward mapping the roads for such an exploration, let me cite two phenomena.

The first is the plight of the "guest workers" in Western European social democracies. Whether they come from Africa, Yugoslavia, Spain or Italy, they are permitted to stay only for brief periods of time, are generally not allowed to bring their families, and are vigorously excluded from citizen-

ship. Such practices seem so incredible to Americans that we simply ignore them as if they didn't exist. We are told repeatedly, for example, how "progressive" and "enlightened" the Swedes are and how much we have to learn from them. Yet for all their progress and enlightenment, they are not about to treat Italian guest workers like anything more than outcasts who are not especially welcome, and surely they will never be permitted to become Swedes.

Like so many other things in American society that are taken for granted, no one has thought it particularly worthwhile to understand why citizenship is so readily accessible in the United States to immigrants, when in most other North Atlantic countries it is but rarely conceded to foreigners and then only under the most rigorous circumstances.

Core of Beliefs

As Arthur Mann at the University of Chicago has suggested to me, the founding fathers of the United States, political philosophers that they were, were very conscious of the need for an intellectual and cultural base for their new nation. Such a base could not be religious because the society was already denominationally pluralistic: Congregationalist in New England, Quaker in Pennsylvania, Anglican in New York and Virginia, Methodist and Baptist in the South. Nor could the cultural basis for the society be ethnic. Even at the time of the Revolutionary War at least half of the population was not Anglo-Saxon. (Most of the non-Anglo-Saxon half were Scotch-Irish, German, or black.) Nor could the common basis be a unique cultural heritage, for while the Battle of Hastings, the Magna Carta, the War of the Roses, and the Glorious Revolution meant something to the Anglo-Saxons, it meant much less, if anything at all, to the non-Anglo-Saxons.

Therefore the founding fathers decided, as the early naturalization laws make clear, the central core of beliefs that was to create the American nation must consist of certain political principles as contained in the Declaration of Independence and the Constitution. Citizenship would be granted to the man who was willing to be a "citizen" in the Enlightenment sense of the word. No man who committed himself to the political principles of the eighteenth century, and who was willing to pledge allegiance to them as they were enshrined in the Declaration of Independence and the Constitution, could be excluded from citizenship whatever his religion, his ethnicity or his heritage.

I suppose Jefferson and Madison would have been horrified at the thought that within something less than a century, while the population expanded across the continent, 45 million new immigrants would come to the shores of the United States from all over the world. However grudgingly, the native Americans did indeed admit the immigrants, requiring (in theory at least) only that they pledge al-

legiance to the political system in order to achieve equal rights as citizens. The system may have been flawed, but it was flawed in practice, not in theoretical statement. The incredible thing is not that there has been injustice and violence in the history of the United States, it is that the country has held together at all.

Losing Nerve

But let us be clear about the flaws. Neither the blacks nor the American Indians were given an opportunity to become citizens. Orientals were admitted for a time but then excluded. Eastern and southern Europeans were admitted by the millions, but then the American republic lost its nerve and, departing from its principles of equal access to citizenship, established quota systems to keep the "inferior" peoples of eastern and southern Europe from contaminating the native American stock. German-Americans, whose loyalty to this country should never have been questioned in either 1916 or 1941, were forced to pay a heavy cultural price for being German. Japanese-Americans were herded together in concentration camps during World War II. Finally, while in theory it was not required of immigrants that they give up either their own language or their own culture, in practice the social pressures were so strong that languages were lost and cultures were repressed.

But the American creed kept us uneasy about these transgressions. The Immigration Act of 1965 eliminated quotas against Orientals and eastern and southern Europeans. While injustice against blacks and American Indians remains, practically no one in the society defends it any longer, and major efforts are being made to eliminate it. More recently, in great part as the result of black emphasis on cultural diversity, the country has at last begun to come to terms with the religious, racial, ethnic, and geographic diversity that exists within its boundaries.

Despite all its flaws, then, the American experiment in pluralism has been in many ways an incredible success. When one looks at the ethnic, religious and racial conflicts in Indonesia, Ceylon, India, Bangladesh, Iraq, Burma, Cypress, Palestine, Yugoslavia and Ulster, one is astonished that there has been so little conflict and violence in American society. Despite its large population, its immense geography and the variegated origins of its citizenry, the United States has had only one civil war, and that was a conflict between two basically British-American groups. Scotch-Irish and Celtic-Irish in the United States get along reasonably well, while in Ulster they still shoot at one another. The United Kingdom may be a far more civilized place than the United States, as many of our self-critics are only too happy to remind us, but that Celt and Saxon are at peace with each other here surely must be considered some sort of progress over the situation in Ulster.

The second phenomenon can be described more briefly. In research currently underway, my colleagues and I have been investigating changes of political attitudes and opinions in American society over the last two decades. Surprisingly enough, on a multi-item scale there has been little change in the proportion termed "liberal" in 1956 and 1968. Using this scale dichotomized the population in 1956; 48 per cent were "liberal" in 1968, a decline of only two percentage points. What this small change masks, however, is a considerable change at both ends of the scale: that is, that the proportion "very liberal" and the proportion "very conservative"have both increased. But they increased in equal proportion, thus leaving the mean unchanged even though the "center" position has eroded.

But this phenomenon is not replicated among the ethnics. While half of them were "liberal" in 1956, 67 per cent were "liberal" in 1968; and while 4 per cent were at the far liberal end of the continuum in 1956, 25 per cent were there in 1968. The general population (white urbanites outside the South, for the basis of this comparison) has remained at the same mean over the period despite the increased size of the poles. At the same time the ethnics have clearly moved to the left—exactly opposite to the "backlash" view of the conventional wisdom—and precisely at a time when the ethnics were notably improving their socioeconomic status.

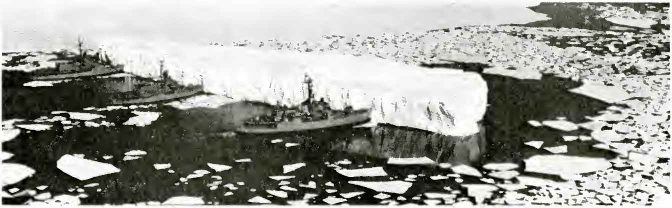
My main proposal, then, is quite simple. We should endeavor to understand American society as it really is and not as it looks from an airplane flying from Kennedy to San Francisco International Airports. Alexis de Tocqueville quite correctly noted the amazing American propensity for self-conscious evaluation of the successes and failures of the republic, an evaluation that, understandable in view of some of the comments I made earlier, was done within the terms of the extraordinary political ideals that bound the republic together.

Binding Forces

What are the forces that bind the society together despite the immense centrifugal forces at work? What are the cultural riches that may be latent in the various subpopulations? What are the aspects of the symbol system of the American civil religion that can be activated to facilitate positive and constructive social change? How does one go about putting together a constructive coalition in American society?

To put the matter differently: We have not been able to accomplish social change of a major sort in the last decade by massive government expenditures, by extraordinarily activist court decisions, by protests and mass demonstrations, by threats of revolution, or by the micropolitics through which power was seized in the Democratic Party. We might now try understanding.

But of course that requires time and energy. Moral denunciations need little of either.



Three icebreakers push an Antarctic iceberg near McMurdo Station. Their combined efforts moved the berg 2.5 miles in 12 hours.

W. F. WEEKS and W. J. CAMPBELL

Water supply problems are clearly becoming increasingly crucial in a number of areas of the world. Unfortunately 85 per cent of the world's available fresh water resides as ice in the Antarctic and Greenland. This water has generally been considered unavailable to portions of the world where large amounts of water could conceivably be utilized. In the present paper, recent ideas for supplying, via the towing of icebergs, large amounts of water in the form of ice to arid coastal areas in the Southern Hemisphere are discussed. It is suggested that the idea appears both technologically feasible and economically attractive and merits serious consideration. *W.F. Weeks, a glaciologist, is a member of the Snow and Ice Branch of the U.S. Army Cold Regions Research and Engineering Laboratory. W. J. Campbell, a geophysicist, is with the Ice Dynamics Project, Water Resources Division, U. S. Geological Survey, University of Puget Sound.*

Towing Icebergs to Irrigate Arid Lands

Manna or Madness?

We recently completed an examination of the idea that icebergs could advantageously be towed from polar regions to more temperate arid regions where they could be utilized as a fresh water source.[1] The origins of this interesting idea may date from the 1850s when lake and glacier ice were transported from Alaska to California as a commercial venture.[2] The current revival of the idea, in particular as applied to the water problems of Southern California, can be credited to John Isaacs of the Scripps Institution of Oceanography.[3] Unfortunately his study was never published. We initially undertook our investigation of the problem because every so often a letter would arrive at the Cold Regions Research and Engineering Laboratory (CRREL) asking, "Why doesn't someone tow icebergs?" We thought that the answer was simple. "Because the idea is untenable!" So we set out to prove that our intuition was correct. It wasn't. Indeed, we now believe that the idea is highly attractive when applied to selected locations in the Southern Hemisphere. The only consistent sources for the large tabular icebergs required for towing are the ice shelves of the Antarctic. The tabular shape is desirable to minimize the hazards of rolling. Admittedly large tabular icebergs calve at irregular intervals from the Ellesmere Ice Shelf, producing the so-called ice islands of the Arctic. These icebergs, however, largely remain in the Beaufort Sea Gyre and only occasionally exit into the Greenland Sea from where they could be towed.

Prime iceberg production sites in the Antarctic are the Amery and the Ross Ice Shelves which could respectively supply icebergs for tows to Australia and the Atacama Desert region in Chile on the western coast of South America. The icebergs that

calve from these shelves have a mean thickness of roughly 250 m and a mean density of 850 kg/m^3. Available data on the distribution and the sizes of such icebergs indicate that the supply is more than adequate.[4] Although in our towing analysis we will consider that the icebergs will be towed from the edges of the shelves, many icebergs have, in fact, been sighted north of 50°S. Therefore, one would attempt to select the optimum combination of iceberg size, shape and location. The current availability of high quality imagery from the Earth Resources Technology Satellite (ERTS) should greatly assist in the selection of such suitable icebergs. In short, available information indicates that the icebergs are there; the problem then becomes one of arranging transportation.

Towing Tests

To estimate the force required to tow tabular icebergs of different sizes we simply calculated the total drag at different velocities. The principal uncertainty in this calculation is caused by the uncertainty as to the appropriate value of the form drag coefficient. Based on drag coefficient determinations on blunt ended objects,[5] we used 0.9 to calculate the sizes of icebergs that can just be towed by a given tug, and 0.6 as the average drag coefficient during transit. Recent towing tests on icebergs located in the North Atlantic indicate that these values are reasonable. The change in the drag coefficient during towing is, of course, caused by differential ablation which rapidly streamlines the iceberg.

An examination of the relative contribution of the form drag and the skin friction drag indicates that in iceberg towing, the form drag portion greatly predominates. This is in sharp contrast to conventional ship hull forms where skin friction is 70 to 80 per cent of the total drag. Inasmuch as it is clearly advantageous to select the most "ship-shape" iceberg that is available, in the remainder of this paper we will always consider rectangular icebergs with a length/width ratio of 4. This is the largest length width ratio that can reasonably be expected to occur commonly in a sample of shelf icebergs.[6] Even for such "streamlined" icebergs, the total drag is very large (Figure 1).

Now that the drag is known, the requisite power required of a tug can be calculated by using a relation which was developed to relate the static bollard pull force that a tug can develop to its power, the diameter of its propeller and dimensionless thrust and torque coefficients.[7] A large amount of power is required for even a small iceberg. For instance, if we assume that 0.25 meters per second is the lowest towing speed at which control can be maintained over the tow, then the world's largest operational tug (the *Oceanic*, power 1.3×10^7 watts) can only tow an iceberg with the initial dimensions of 230 x 920 x 250 m ($5.29 \times 10^7 m^3$ of ice). Assuming this iceberg could be delivered without losses, its water would be worth $8.6 million at the current estimated production cost of large-scale desalinated water ($0.19/$m^3$ of water).[8] A hypothetical 1.56×10^8 W super-tug, that could be built with present marine technology, could tow icebergs with widths of 2.8 km and 750 m at 0.25 and 0.50 m/s respectively. On delivery these icebergs would be worth $1,254 million and $91 million. Clearly, economically interesting iceberg tows are within the capability of current technology. (The nuclear aircraft carrier *Enterprise* has 50 per cent more power than our proposed super-tug.)

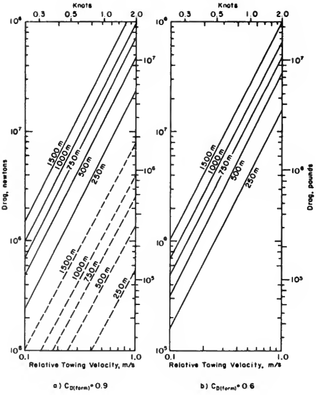


Fig. 1—Drag of rectangular icebergs with length/width ratios of 4 and form drag coefficients of 0.9 (Fig. 1a) and 0.6 (Fig. 1b), respectively. The dimensions given are for the iceberg widths. The iceberg drafts are assumed to be 200 m. The total drag is indicated by the solid lines and the skin friction drag by the dashed lines. The skin friction drag is the same in Fig. 1b as in Fig. 1a.

Melting Losses

To obtain the above large numbers, we cheated by neglecting melting losses. Because transit times at reasonable towing speeds are in excess of 100 days and water temperatures at the delivery sites are in excess of +15° C, melting losses are quite important. Unfortunately, even if a detailed series of calculations were made, there are no adequate field observations on icebergs on known geometries with which they could be checked. Therefore we have simplified the problem by calculating the parallel recession of the iceberg sides based on an average heat transfer coefficient determined from experi-

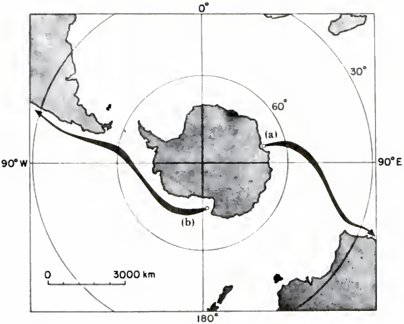


Fig. 2—Optimum towing paths between the Amery Ice Shelf (a) and Australia and the Ross Ice Shelf (b) and the Atacama Desert, Chile.

mental studies on the fully turbulent flow of fluid over a flat plate.[9] We also assumed that the iceberg is completely immersed in the water. Although this is, of course, not true, it should nevertheless give a reasonable approximation to reality because undercutting of the submerged portion of the iceberg will cause the unsupported upper portion to calve. Austral summer water temperatures were used in these calculations as a built-in safety factor. Also the fact that the initial average temperature of an iceberg near the edge of the shelf is roughly -10°C was ignored.[10]

Sea Routes

To estimate the amount of melting, we first calculated the losses for great circle routes and assumed that the towing speeds were constant (i.e., we ignored the geophysics of the situation). The results indicate that in-transit melting losses are quite large and are excessive for small icebergs; losses of over 100 m of ice from each face are common. Fortunately the total amount of ablation is not a sensitive function of transit time (as the transit time varies by a factor of 40, the ablation only doubles). Also the larger the initial iceberg, the less the ablation on any given face. Clearly if large enough icebergs can be towed, large volumes of ice can still be delivered.

Great circle distances, although the shortest, are clearly not the easiest because of the winds and currents along the routes. Also as the iceberg is towed north, its size decreases because of ablation, permitting higher towing velocities. To properly estimate towing times, routes and the amount of ice delivered, all these factors must be considered simultaneously. We, therefore, solved the basic steady state equation containing the towing force, the air stress, the water stress (drag), the Coriolis force and the gradient current force simultaneously with the melting equation. The final transit routes were chosen by maximizing the velocity for the overall route. The wind velocity and gradient current fields that were used were the seasonal mean fields from the U. S. Weather Bureau Antarctic Summary and the Russian Antarctic Atlas. The optimum paths for the Amery-Australia and Ross-Atacama (Chile) routes lie in the well defined envelopes shown in Figure 2. It was found that even considering in-transit melting, the highest transit velocities occurred during the middle portions of the tows because of the high winds and gradient currents during these portions of the trips (Figure 3). Also as anticipated by our earlier simplified analysis of melt-losses, major changes in the towing capability of the tug made only minor changes in the amount of ice delivered during a given tow. Changes in the towing force do, however, make significant changes in the volume of ice delivered per year because of the shorter transit times involved per iceberg. As expected, the delivery rate is appreciably higher on the Australian than on the Atacama traverse. However, in both cases the volume of ice that can be delivered is very large.

A complete economic analysis of icebergs as a fresh water source is difficult in that it depends on a number of factors, many of which are peculiar to the delivery site that is selected. An apprecia-

tion of the complexity of this problem can be gained by studying recent planning documents that discuss the feasibility of large-scale desalination projects in different parts of the world.[11] We will circumvent this difficulty by only appraising the economics of transporting the icebergs to the delivery site. If this is inexpensive relative to the cost of fresh water, then this fact should serve as an incentive for further research on the development of an overall iceberg water supply system. We will also discuss iceberg towing as if it were a wholly private venture. This does not mean that we think that this would be the case. The large amounts of land required by such a project would surely necessitate at least partial government participation.

First, the total operating costs of several existing and hypothetical tugs were estimated and used to develop a curve relating tug power to total operational cost. The fact that design studies have been completed on some very highly powered ships for potential Arctic operation was a great help here.[12]

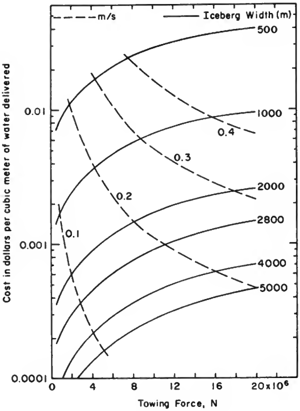


Fig. 3—Changes in iceberg volume V, iceberg thickness x_t, and transit velocity v_t as a function of distance traveled along the towing routes between the Amery Ice Shelf and Australia and the Ross Ice Shelf and the Atacama Desert. The initial iceberg dimensions are 2800 x 11,200 x 250 m and the towing force is 18.4 x 10^6 N.

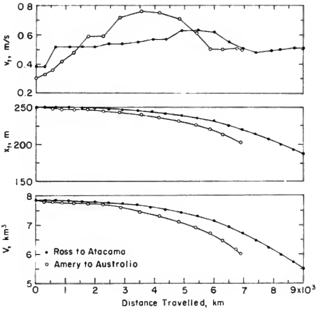


Fig. 4—Delivery cost per cubic meter of water delivered as ice to Western Australia expressed as a function of towing force and initial iceberg width (initial length = 4 x initial width). The dashed cross-curves give the sizes of icebergs that can be towed at the indicated steady-state velocities as a function of the towing force if a form drag coefficient of 0.9 is assumed.

Then knowing the ice delivery rate per year for several different tugs, curves were plotted for a variety of initial iceberg widths to show the delivery cost of ice, expressed in terms of the equivalent amount of water, versus the towing force. Figure 4 shows such a plot for the Amery-Australia transit. Here it should be remembered that a towing force of 1.3 x 10^6 newtons (N) corresponds to the *Oceanic*, 6 x 10^6 N to a tug with powering similar to the U.S. Coast Guard Icebreaker *Polar Star*, and 18.4 x 10^6 N to a 1.56 x 10^8 W super-tug. This figure reveals a great deal about the economics of iceberg transportation.

Although delivery costs appear amazingly low, one must realize that the complete range of these values is not available; if the towing force is so small that only very low speeds can be obtained, the tug will not be able to control the tow. This restriction is indicated in the figure by the dashed cross-curves which give the sizes of icebergs that can be towed at the specified steady-state velocities as a function of the towing force. Therefore only prices above the appropriate minimum control speed cross-curves are available. We do not know what the lowest possible control velocities are, but their values for the large tugs and tows considered here are clearly critical. If, as earlier, we assume a minimum control speed of 0.25 m/s and

use a super-tug, delivery costs for water delivered as ice to Australia and the Atacama Desert would be 1.3 and 1.9 mills/m^3, respectively. In general, to minimize delivery costs we should use as large a tug as possible and tow the largest possible iceberg that still allows us to maintain control.

How much is this water worth? Water prices, of course, vary widely. Inasmuch as iceberg towing appears most attractive when large volumes of ice are involved, we feel that the economics of iceberg water should be based on the price of irrigation water. A realistic price for irrigation water is, in itself, difficult to establish. Perhaps the U.S. federal price goal for irrigation water produced by desalination should be used—\$0.03/m^3. However recent studies suggest that even this value is high and that 8 mills/m^3 is a more realistic planning standard.[13] This price is similar to the cost of producing groundwater from a large, high quality well. Therefore our analysis suggests that between 6 and 4 times the cost of delivering the ice to the Australian and western South American sites could be used to process the ice and still have the final water cost remain highly competitive. Inasmuch as our analysis is in general a "worst case" analysis, this suggests to us that iceberg water is indeed an attractive idea. At least different aspects of the problem should be examined by groups of specialists.

Problems and Difficulties

It is easy to compile a long list of problems. The towing analysis could be greatly improved. Accurate drag coefficients are particularly needed inasmuch as their values determine the size of the iceberg that can be "started" and much of the resulting economics. Our method of calculating melting losses should be experimentally verified and expanded to take the change in shape with melting and its effect on towing into account. The problems related to the anchoring of tow lines should be investigated. Methods for reducing melting losses should be examined. This is particularly important if tows to more northern locations are contemplated.[14] More exact information is needed on the difficulties of controlling the tows. Can calving be expected to be a major problem? It would also be desirable to have an economic analysis based on nuclear-powered tugs as opposed to the more conventional propulsion systems we have considered.

In addition the range of approaches and problems related to processing the ice should be thoroughly explored. We have only discussed this area in a qualitative way. One interesting aspect of the processing of the ice is that the iceberg is an immense heat sink. Therefore if an iceberg towing scheme were coupled with any operation that generates large amounts of waste heat, such as nuclear power generation, this heat then becomes a valuable asset in the conversion of ice to water. Another interesting problem is assessing the environmental impact of such a scheme on the delivery site; what climatological, oceanographic and biological changes can be expected?

Our present work merely provides a first approximation to a quantitative appraisal of the availability and transportation aspects of icebergs as a fresh water source. The possibility of irrigating large areas of arid land in the Southern Hemisphere is certainly desirable. For instance, the amount of water produced by one super-tug alone is roughly four times the amount produced by the Snowy Mountains Project[15] and would irrigate a square field 126 km on a side. The best part of the scheme is that its principal commodity, the iceberg, is currently being completely wasted as regards man's needs. The icebergs calve from the shelves and drift in the Southern Ocean until they melt. The towing proposal merely redirects this water through an irrigation system on its way to the sea. We would guess that the potential rewards to man of the more tortuous path will prove to be well worth the additional energy expenditure.

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The President's Plan

(Continued from page 8)

Second, NSF operates under a modified law which gives it increased responsibility for applied science and which has increased the number and stature of its senior staff (six Presidentially-appointed staff members, instead of the previous one). In Guyford Stever, NSF has an experienced and knowledgeable Director whose stature will be significantly strengthened by the additional formal title of Science Advisor. It is reasonable, therefore, to expect that NSF will be able to do an effective job on at least *some* of the responsibilities of OST and PSAC, and at least *some* of the responsibilities of the previous full-time Science Advisor.

One may go farther and point out that NSF, at least in the area of study and analysis, offers some potential advantages over OST. The NSF staff is considerably larger with a broad spread of talents. Furthermore, it contains several groups whose assignments are such that they might quite reasonably undertake additional nationally-oriented study efforts. Thus, within the Research Applied to National Needs (RANN) program there is a sizeable group concerned with advanced technology applications. There is also a comparatively new Office of Problem Assessment. Finally, the NSF Division of Science Resources Studies has been involved for some years in establishing an adequate data base for scientific and technological activities within the United States, and this data base will be invaluable to many of the desired studies.

However, many of the older and fundamental difficulties remain to plague the NSF, along with perhaps some new ones. In the first place, the problem of serious study, overview, and evaluation by one "science agency" within the federal government over the science programs of other, often much larger, federal agencies cannot fail to be a delicate and difficult one. Furthermore, NSF is an operating agency with a program of its own, and to confuse and even conceivably endanger this operating program by assigning NSF some touchy coordinating and overview responsibilities will be awkward, to say the least.

Thus, in spite of NSF's greater size and competence, it is difficult to believe that it will *really* attack the problems of evaluation and coordination, elimination of program overlap, recommendations for new and gap-filling programs which is implied by "taking over the functions of OST." As only one example, will NSF be willing to move strongly into the field of water resources to help resolve the overlapping and duplicating responsibilities and activities of such powerful agencies as HEW, the Department of Agriculture, the Department of the Interior and the Atomic Energy Commission? It does not seem very likely.

Of equal importance is the fact that NSF, as of this writing, simply will not have uncommitted funds to take on any very large fraction of the OST activity, and will necessarily have to proceed by internal transfer of funds and people. The President's message states: "The multi-disciplinary staff resources of the Foundation will provide analytic capabilities for performance of the transferred functions." Given that NSF is already an overworked and understaffed agency, this seems either ingenuous or misleading. Furthermore, by no means in all cases will the capabilities of the NSF staff, hired for different sorts of jobs, fit the analysis and evaluation needs which some of the OST functions will call for.

Science advising for the White House has always been concerned with two somewhat different problem areas. One is policy for science; the other is policies for application of science and technology to societal problems. The first is, in many ways, the simpler area; but it still involves questions of level of support for basic research, policies for graduate education, etc. In recent years, the Office of Management and Budget has maintained a strong overview of this area; this will certainly continue. Stever, as head of one of the major science support agencies, will speak from a position of great knowledge, and he has a real opportunity to be influential with OMB. But even here there are points of awkwardness. First, since Stever reports as Science Advisor to George Shultz, his ability to "sell" recommendations that go contrary to OMB guidelines will not be great. Furthermore, Stever's position cannot help but have elements of ambiguity, since he is simultaneously Director of an agency whose own success is closely linked to the policies for science on which, as Advisor, he will be making recommendations.

The area of policies for applying science and technology to societal problems is more complex. Here the locus of action is in the large mission-oriented agencies. The NSF programs of applied science and use of technology are new and still rather small. Hence, to have an impact on these broader areas (whether by study, coordination or evaluation), NSF will need to build both strength and competence, and ultimately to persuade both OMB and the affected agencies of its capabilities. Since many of the areas are controversial and have large elements of political sensitivity, this is bound to be a troublesome field for NSF.

With only modest restructuring and funding, one could imagine NSF moving vigorously in a somewhat less burdensome area which was once

a province of PSAC, that of establishing study panels of outside experts to make continuing analyses of areas of science and technology of importance to the nation and to the federal government. Working with outside panels is a field where NSF has background and experience. It has effectively utilized advisory committees and review boards for most of its program areas and for its principal offices. It would not be a major extension of these activities to undertake PSAC-type panel studies. There are, however, some difficulties even here. In the first place, many of the areas which PSAC and OST studied were difficult and controversial (e.g., World Food Supply, Civil Defense, Supersonic Transport, Strategic Systems of the Military). Given that studies in these and many similar areas are more than likely to raise internal problems and interagency conflict, it will always be difficult for an "exposed" agency like NSF to make this kind of study. The new laws on government advisory committees, requiring them to operate in full public view, will exacerbate this difficulty. One strongly suspects that at best NSF will be able to undertake only a portion of this PSAC activity.

From this analysis of the potential role of NSF, my conclusion is that without considerable reorganization and funding NSF will be able to carry out only a small portion of the activities previously undertaken by OST, PSAC and the Science Advisor and his Deputy. Even with some new funds and much vigor and initiative on the part of NSF, there will be areas of awkwardness and difficulty which will seriously inhibit the NSF. Finally and unavoidably, the studies and recommendations of NSF, as one federal agency among many, will simply never carry the "clout" that could come from studies made under a Science Advisor sitting within the White House family. In other words, there are losses to the nation and to the federal programs in science and technology from the abolition of the White House science advisory activity which

cannot fully be recovered—even if the NSF operates with the best will and vigor.

There is, however, an even larger question. Do the science community and the nation really want the NSF to apply itself vigorously to these new activities? After all, the responsibilities already assigned to the NSF—support of basic and applied science, programs of technical innovation and of application of science to social problems, new directions for science education—are important in themselves, and the people who strongly hope to see these done well may be quite unwilling to see NSF embark into new and potentially controversial fields. And make no mistake, the problems of science advising, if done seriously, and of overview and coordination of federal programs are difficult and controversial. Even with vigorous Presidential protection, the actions of a lively Science Advisor, by the very act of making strong recommendations on controversial problems, cannot help but erode his support; it is surely no accident that the average tenure of Science Advisors has been significantly under three years. Do we really want to see NSF and its Director committed to these perilous waters?

Inescapably, the key figure in all this is the NSF Director, Guyford Stever, who has been assigned the "functions of OST" as well as the additional title of Science Advisor. The difficulties of "wearing several hats," which in some degree plagued the former Science Advisors, will now descend on Stever. But in contrast to the relatively protected position from which the previous Science Advisors operated, Stever, as head of an important federal agency, will necessarily operate in an atmosphere of continuing public view and Congressional concern. His friends and those of NSF can only wish him well as he starts to walk the looming tightrope.

Actually, from a different and somewhat Machiavellian point of view, it is possible to imagine that a quite different kind of science advisory system is waiting to be born, one which would give much less responsibility to NSF. Such a scenario is implicit in some of the more sweeping Presidential restructuring of the Executive Branch, which is having an impact on almost all federal departments and agencies. An admiring spectator of the major reorganization now going on in the Executive Branch has put the case somewhat as follows:

> The consequence of the Presidential reorganizations will be to replace the diffuse and overlapping agency structure which has characterized Washington in the recent past by a much more tightly organized group of four principal activities, each headed by a super administrator who will effectively serve as a program manager for his area. One area is economic growth and industrial development, as currently headed by George Shultz. A second area is problems of foreign policy and national security, under Henry Kissinger. A third is natural resources, headed by Earl Butz. The fourth area might be called human resources with Caspar Weinberger as the chief. One can assume, or perhaps hope, that each of the new program managers will develop his own study and coordinating mechanism to bring science and technology into his area. When these new structures are in place, each super-department head will have a science advisory system more closely linked to his actual decision-making needs and consequently more effective.

But as we watch and wait to see if this suggested new structure takes shape, we still must note that it will in no way provide the overview and coordinating efforts which were the principal raison d'etre for PSAC, OST and the Science Advisor. Nor will it provide an effective procedure for bringing to the President integrated position recommendations in areas where the "super-departments" are in conflict.

I am forced to conclude that, in the abandonment of the science advisory structure which has been located in the White House, the nation has suffered a genuine and potentially serious loss, one which virtually none of the currently discussed responses will fully overcome. Unhappily, this loss occurs at a time when the national needs for technology are more, not less, and when the problems to which technology should be applied look increasingly difficult, verging in some cases on the intractable.

Let me close this analysis of the Presidential Reorganization Plan No. 1 with a few remarks on a quite special area of science and technology, that of military technology. This is a subject which has deeply concerned PSAC almost from its beginning; PSAC panels dealing with military programs for many years have been a source of independent analysis and advice (by no means always accepted) on such diverse military questions as strategic military systems, military space activity, intelligence gathering, naval systems and submarine warfare and new weapons systems. In its discussion of Reorganization Plan No. 1, the White House comments on this area by saying: "In the case of national security, the Department of Defense has strong capabilities for assessing weapons needs and for undertaking new weapons development and the President will continue to draw primarily on this source for advice regarding military technology." Given the historically narrow point of view which has formed the basis for military advice, this statement is not very reassuring. My own strong belief is that both the federal government and the U.S. general public badly need a vigorous and continuing *independent* capability for analysis of military proposals, most especially those for new military technology. To paraphrase an ancient remark, "military programs are too important (and too expensive) to leave to the military." It is conceivable, but not very likely, that the National Security Council under Henry Kissinger will address this problem and establish significant independent analyses of military technology. In the meantime, one can only note that here is a time and place where the non-governmental systems of study and analysis had better work doubly hard.

BOOKS — *Science and Public Affairs*

Citizens vs. Atomic Power

"The Nuclear-Power Rebellion: Citizens vs. the Atomic Industrial Establishment," by Richard S. Lewis. Viking Press, New York, 1972. 313 pages, $8.95.

Reviewed by
DEAN E. ABRAHAMSON

A controversy has evolved over the acceptance of civilian nuclear energy. The issues involved are technically diverse and socially and politically grave. This book eloquently chronicles a portion of that struggle. Richard S. Lewis in *The Nuclear-Power Rebellion* has defined the promoters as composing the Atomic Industrial Establishment (AIE):

> Because of this circumstance [the tight secrecy surrounding atomic technology until the early 1950s], civilian atomic power, beginning as a government project, inevitably became a joint enterprise of the agency which controlled access to the technology and the electric-power industry which could apply it to public use. The framers of the Atomic Energy Act of 1954 set up the partnership. "Teamwork between government and industry is the key to optimum progress, efficiency, and economy in this area of atomic endeavor," the Joint Committee on Atomic Energy reported in offering the bill to Congress. Under the Act, the AEC [Atomic Energy Commission] acted as the promoter of the Promethean gift and was also responsible for regulating its use in the public interest. The arrangement implied a strong conflict of interest, in which the public interest was submerged. It is a maxim in Washington that regulatory agencies tend to identify more closely with what they are supposed to regulate than with the public they are supposed to protect. In the regulation of nuclear power, the identification between the AEC and the industry, together with its satellite trade associations, amounted to a commonality of interest and purpose. It was not surprising, therefore, that in the later 1950s there evolved a joint government-industry power structure with common goals and with the access to money and political power to achieve them. I have suggested that this amounts to an Atomic Industrial Establishment devoted to the task of building a fission-reactor power economy, eventually to be based on plutonium fuel, that will last for an indefinite future. In this Establishment the AEC functions as banker and engineer and the Congressional Joint Committee on Atomic Energy as *de facto* board of directors. With the wealth and authority of the government and the investor-owned electric utilities, the Atomic Industrial Establishment is well on the way toward dominating the electric-power industry and shaping the energy future of America.

In no other instance have the basic axioms of the promoter been more clearly illustrated than in the case of atomic power. In no other instance has it been more clearly demonstrated that the promoter needs to be challenged. And in every case the challenge has been made by individuals who, often at great personal sacrifice and risk, have shown that the AIE has been technically incompetent, lacking in candor and honesty, and incapable of protecting the public health and welfare.

On no other issue has the league of regulator and regulatee been so clearly shown. In no other instance has it been more evident that there can be no effective assessment of hazards without issues being raised and remedies provided by independent scientists working with an active and concerned public.

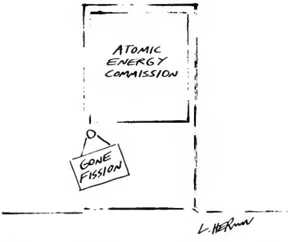
Such is the stuff of which the nuclear-power rebellion is made and which has been examined by Lewis in this timely, important and incisive book.

Except for an occasional historical review to put a specific topic into perspective, *The Nuclear-Power Rebellion* deals with a few of the many recent examples of citizen involvement in clashes with the AIE. Only passing reference is made to the earlier involvements with weapons testing and the early reactor and Plowshare cases. The stage is set with an example of citizens becoming involved, a terse review of some of the issues, and a concise background review showing how:

> The Atomic Industrial Establishment was confronted by a determined opposition which began to consider the AEC as a public enemy.

The public's challenge to the reactor program, in contrast to earlier skirmishes over specific reactors, began in about 1968. It drew, however, upon the expertise and concern nurtured for many years by scientists, lawyers and economists who became involved as well as upon experiences resulting from earlier reactor cases, particularly at Enrico Fermi in Detroit and Bodega Head in California.

Although most of the obvious issues associated with the "peaceful" atom have now been identified and brought into the light of public view, not all have been publicly debated. Those issues which have received attention include the apparent inability of the AEC to pursue its regulatory charge with the same vigor with which it pursues its charge to promote nuclear power; the familial relationship which has developed between the Joint Committee on Atomic Energy (JCAE) and the rest of the AIE; the importance of ready access for citizens in licensing and rule-making proceedings; thermal pollution; routine releases of radioactive



wastes; the adequacy of radiation exposure standards; disposal of high-level wastes; uranium miners' health and safety; and the adequacy of emergency core cooling systems.

Issues which have finally been raised but have not as yet been explored in any detail include the adequacy of plutonium standards and regulations; the ability of the AIE to handle plutonium; the general question of plutonium toxicity; the decision to develop the liquid metal fast breeder reactor (LMFBR); safeguards and diversion of plutonium into unauthorized channels, either national or sub-national in character; and finally the degree of social and political control which will be required by a mature atomic industry based on plutonium. These latter issues are only now beginning to be discussed in public fora. They are the most important ones—those which expose the profound implications of "clean, safe nuclear power."

Lewis has concentrated on only a few of these issues, for his intent was not to provide an encyclopedic compendium of the problems associated with atomic power but rather to demonstrate that:

> If the environment were to be protected, citizens would have to do it; that if public health and safety were to be guaranteed, citizens would have to see to it. The agency to which these responsibilities had been entrusted was busy serving the interests which it was supposed to be regulating.

He has succeeded in this demonstration and done so in a convincing and concise manner.

The specific elements of the nuclear controversy that are examined in *The Nuclear-Power Rebellion* include the evolution of a little-known citizen's effort in Pennsylvania over a proposed breeder reactor near Meshoppen; the national debate over the adequacy of radiation standards—Lewis outlines in some detail the effective participation of John Gofman, Arthur Tamplin and Ernest Sternglass; the effective use of citizen intervention in licensing hearings to challenge routine releases of radioactive wastes, whereby forcing the issue of cost versus radioactive releases into the light of public scrutiny; the proposed Lyons, Kansas, scheme for disposal of high-level radioactive wastes in a salt mine; the sea level canal and gas stimulation Plowshare projects; the onset of the challenge to the LMFBR program; and finally the Calvert Cliffs decision which found the AEC violating the National Environmental Policy Act of 1969.

These cases are handled in an objective fashion and by and large are complete and accurate. If I were to make any generic criticism of the way in which these cases are presented, it would be that while the roles of independent scientists are fully drawn, Lewis does not give adequate credit to the major roles played by the attorneys involved in many of them. For example, in the presentation of the LMFBR case he does not acknowledge that the attorneys of the Natural Resources Defense Council, principally Gus Speth, were largely responsible for laying out the challenge in its present form.

It might be of interest to bring up to date some of the cases and examples cited in *The Nuclear-Power Rebellion*. First the general thesis—if the environment and public health and safety are to be protected, citizens will have to do it—is as true now as it was in the cases considered by Lewis. Also, individuals, particularly those in the AEC's own laboratories, who choose not to follow blindly AIE policy are still subject to harassment and pressure, and their work to the supression described by Lewis. For example, during the successful challenge to the adequacy of radiation standards Tamplin's group at the Lawrence Livermore Laboratory (LLL) was reduced from 12 persons to 1, Donald P. Geesaman. In April 1973, Geesaman was discharged and the group was abolished entirely.

The emergency core cooling system (ECCS) hearings in Washington, D.C., brought to light AEC reprisals against staff members of the AEC and also of its contractors who ventured to express their reservations about the adequacy of the current ECCS criteria. Morris Rosen was "promoted" so that he no longer would have direct responsibilities for ECCS work (see, e.g., *Nucleonics Week*, March 2, 1972). An Oak Ridge National Laboratory (ORNL) memorandum which cast doubt on ECCS criteria was suppressed (*Nucleonics Week*, March 16, 1972).

Later in the ECCS hearings charges of reprisals against witnesses in the ECCS hearings apparently prompted Alvin Weinberg, director of ORNL, to release a letter expressly stating that members of the ORNL staff should feel free to state any qualms they had about criteria (*Nucleonics Week*, March 23, 1972). Among other things, Weinberg's letter begs the question as to what part the director of LLL and the Regents of the University of California who operate LLL under contract for the AEC have played in the harassment and pressures yet in evidence at that laboratory.

The ECCS issue was not covered in *The Nuclear-Power Rebellion*, as it had only begun to mature when the book went to press. This case, which has been discussed in a series

of articles in *Science* within the past year has once again demonstrated that the AIE cannot be depended upon to protect the public health and welfare without determined efforts of independent scientists. In this case the Union of Concerned Scientists has performed a public service in raising and documenting a major facet of nuclear safety and forcing the AIE to take notice of it.

What of the future? As Lewis was completing his book a new chairman of the AEC, James R. Schlesinger, had recently been appointed. Schlesinger, through his initial words and actions, stimulated a great deal of hope that at long last the AEC would take its nonpromotional charges seriously. Schlesinger has come and gone, leaving these hopes unfulfilled, and in his final appearance before the JCAE he cast serious doubt on the assumption that the AEC had learned anything during the past years.

A further, and very significant, piece of evidence has just recently emerged from the AEC. A draft report entitled "The Safety of Nuclear Power Reactors and Related Facilities" (WASH-1250, Dec. 1972) purports to be a complete and candid review of the nuclear fuel cycle. Safeguards and diversion are not discussed; plutonium toxicity is not discussed. The discussion of hazards of exposure to ionizing radiation bears little evidence that the authors of this report have been awake for the past several years.

There is no mention of WASH-740 ("Theoretical Possibilities and Consequences of Major Accidents in Large Nuclear Power Plants," 1957) and maximum potential accidents. There is no discussion of the Price-Anderson Act and its implications. These are only a few of the shortcomings of WASH-1250. And this is the report which states that its purpose is to "inform the public . . . to place before the public, hopefully in a manner understandable to the reasonably informed layman, a fairly comprehensive statement of fact, along with some history and policy considerations, regarding the safety and environmental aspects of nuclear power reactors and their support facilities."

It is my opinion that the cases discussed in *The Nuclear-Power Rebellion* are only opening skirmishes in the efforts of citizens to protect themselves from the actions of the AIE. The issues addressed to date—the adequacy of radiation standards, routine releases of radioactive wastes, storage of high-level wastes, and the matter of protection against major accidents—are certainly major issues, and issues which pose enormous hazards. Except for modern weapons, they present more serious hazards than those present in any other industry or human activity. Even so, however, they are of less concern than the issues which only now are coming into the nuclear debate, namely the implications of the need for near-totalitarian control, as the nuclear industry expands, of the inability of industry to successfully contain plutonium, of the ability of terrorist groups to obtain and release plutonium and, finally, of the possibility of diversion of plutonium and other special nuclear materials into unauthorized channels, national or sub-national, for the fabrication of nuclear bombs.

Increasing numbers of individuals appear to be coming to the conclusion that the AEC is utterly incapable of acting to protect the public health and safety or to act as even a brake on the uncontrolled proliferation of nuclear facilities. The publication of WASH-1250 should serve to dispose of any lingering doubts. In his cogent analysis of the Vietnam conflict David Halberstam observes:

> Nor had they, leaders of a democracy, bothered to involve the people of their country in the course they had chosen: they knew the right path and they knew how much could be revealed, step by step along the way. They had manipulated the public, the Congress and the press from the start, told half truths, about why we were going in, how deeply we were going in, how much we were spending, and how long we were in for. When their predictions turned out to be hopelessly inaccurate, and when the public and the Congress, annoyed at being manipulated, soured on the war, then the architects had been aggrieved. (Halberstam, *The Best and the Brightest* (New York: Random House, 1972).)

When the history of the rush to atomic power is finally written, it may include a similar, sorry commentary on the developers and promoters of the "peaceful atom."

In closing, I borrow from a talk at the University of Minnesota given by physicist Donald Geesaman of the University of California:

> A complex and sophisticated society must bear the burdens of vulnerability and constraint that are inherent in its technologies. The Atomic Energy Act of 1954 and subsequent legislation has brought nuclear technology into the purview of the commercial sector. Under the civilian nuclear energy program nuclear materials will become vital elements in human commerce. They will be very special elements of commerce, and they will warp many of our existing institutions beyond recognition.
>
> Nuclear fuel is simply not an ultra-high grade of fuel oil: it is something quite distinct and extraordinary. Few people seem to viscerally understand this; one who does is Alvin Weinberg, director of the Oak Ridge National Laboratory, who reflected: "We nuclear people have made a Faustian bargain with society. On the one hand we offer—in the catalytic burner—an inexhaustible source of energy. . . . But the price that we demand of society for this magical energy source is both a vigilance and longevity of our social institutions that we are quite unaccustomed to" ("Social Institutions and Nuclear Energy," *Science*, 177 (July 7, 1972), 27).
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> Another is Hannes Alfven, Nobel laureate, who wrote: "Fission energy is safe only if a number of critical devices work as they should, if a number of people in key positions follow all their instructions, if there is no sabotage, no hijacking of the transports, if no reactor fuel processing plant or repository anywhere in the world is situated in a region of riots or guerrilla activity, and no revolution or war—even a 'conventional' one—takes place in these regions. The enormous quantities of extremely dangerous material must not get into the hands of ignorant people or desperados. No acts of God can be permitted" ("Energy and Environment," *Bulletin*, May 1972).

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20th Century Scientific Cloisters

"Think Tanks," by Paul Dickson. Atheneum, New York, 1972. 370 pages. $10.

Reviewed by
LOUIS F. GORR

The think tank is the culmination of America's tradition of can-do technology. Yet among the general public it is perhaps the least understood aspect of modern technology. Popular stereotypes of the think tank include everything from the accurate to the fanciful: It is a coven of Strangelovian characters intent on destroying the world. It is a brain trust isolated from the realities of everyday life. It is an elitist conglomerate occupying some ill-defined position between the campus and the corporation. It is attacked from the right as a haven for pointy-headed intellectuals and from the left as a symbol of the military-academic complex. Even the notoriety of Daniel Ellsberg and his Pentagon papers and the student riots on campuses harboring military research units have done little to make the think tank understandable.

Paul Dickson has thus performed a distinct service in writing this book. By assembling a mass of detail about the think tank he has stripped away much of the mystery surrounding it. And he has provided a workable and convenient way of examining its function in the development of modern science/technology.

Other than a few scholarly monographs and a vast number of articles in specialized journals Dickson's book is the only readily available source of information on the subject. That there has been at least a fairly widespread desire for information is evidenced by the fact that this book realized a second printing shortly after its publication. Few books on such arcane subjects as technology and public policy can make an equal claim.

Collectively, think tanks are members of the new knowledge industry. "Clean" factories, they are purely qualitative, and their output and impact are impossible to measure by numerical standards. Think tanks are the bane of the disciples of scientific management and statistical quality control. An automobile manufacturer can point to x-number of cars assembled, and an oil company to y-number of barrels produced. Ideas cannot be measured in such quantitatively simplistic ways. Probably the only gauge—and it is a far from accurate one—of the size of the research industry and its impact is the amount of money spent each year on research and development.

R&D is the ubiquitous term used to describe the think tank. It is a suggestive one and tells a great deal about the think tank as well as about modern science and technology. In modern jargon the two words are used together, seldom separately, and indicate the melding of science and technology. In the modern world the traditional distinctions between science and technology have been blurred. Where once science meant research, and technology meant development, today there is technological research and scientific development. Qualifying terms, such as basic science and applied science, have been coined to describe the new functions of science technology. The think tank has been at the vanguard of these changing roles, and Dickson devotes considerable space to discussing the relationships.

Dickson uses the standard definitions throughout the book: Basic research is the exploration of the unknown, "the desire to pursue knowledge for its own sake." Applied research is directed "toward fulfilling a stated need." Development is "the systematic use of basic and applied research for the creation and production of tangible objects, systems, methods and materials." Thus a chemist trying to find out all he could about a certain compound would be doing basic research. Applied research would begin when he attempted to determine whether the compound would curb a disease, say, cancer. The chemist would enter the development stage when he refined his compound for preparation in mass quantities to control the disease. To varying degrees and in

varying guises all three functions are present in the think tank.

There is an additional function of the think tank however, and that is the exploration of how the findings of science/technology will affect public policy. Investigators in the think tank explore the broadest possible questions: How many citizens can safely and economically be cared for in the aftermath of a nuclear attack? (Herman Kahn's notorious think tank which produced *On Thermonuclear War* asked that question.) How can a new constitution be written for the United States to account for the many changes in government since 1789? (This is a favorite project of the Center for the Study of Democratic Institutions.)

Mother Rand

In 1969 the National Science Foundation determined that there were over 11,000 research institutions in the United States. Twenty years earlier there was but one—the Rand Corporation (an acronym for research and development), called "Mother Rand" by Dickson because it was the prototype of all the following think tanks. But not all research institutions can be called think tanks. According to Dickson, the think tank is distinguished from other, superficially similar institutions by its operation. An ordinary research institution has a specific goal or set of goals; its operation and role are directed toward the achievement of that goal. By contrast, says Dickson in a useful definition, the think tank:

> . . . has no set pattern of financial objectives or affiliation; nor is it a commission or special group with a temporary assignment. The crucial determinant is its role. The primary function of a think tank . . . is neither traditional basic research, applied research nor development—although all three are commonly performed in think tanks—but to act as a bridge between knowledge and power and between science/technology and policy-making in areas of broad interest. They are closer to being agents of new knowledge and discovery than creators of new knowledge.

There are further characteristics of think tanks that set them apart from other research institutions. The think tank is oriented toward the use of scientific methodologies—operations research, systems analysis and the like. Rand pioneered in the development of PPBS—the planning, programming, and budgeting system for efficient management control—for the military. Now PPBS is a commonplace system in many public and private institutions.

The think tank is almost always transdisciplinary and multidisciplinary. The Institute for Defense Analysis, for example, draws from a pool of experts in all the sciences, social sciences, history and mathematics. The think tank has great freedom in defining and solving problems. The celebrated study in the early 1950s by Rand is a classic example of the think tank's freedom. The Air Force asked Rand to determine the most effective distribution of air bases overseas for maximum deterrent capability. The project director, Albert Wohlstetter, began by questioning the validity of the basic assumption—that overseas bases were needed. Mathematically, he and his colleagues discovered that maximum deterrence would be achieved by a network of domestic bases. The Air Force was scandalized but soon agreed with the findings.

Of the 600 think tanks there are about 75 Federal Contract Research Centers. Among others, the Rand Corporation and the U.S. Office of Education's Center for the Advanced Study of Educational Administration are quasi-public organizations, conducting research for a patron governmental agency. One Defense Department official compared these institutions and their connections with the government to that of film stars and their analysts.

Other think tanks exist within the government itself. The White House's Goals Research Staff and the Army's Institute for Land Combat are two of the better known of these. They undertake long-range policy studies and the exploration of future alternatives.

Also qualifying as think tanks are some 200 nonprofit research groups. They are independent or university-affiliated organizations, such as the Battelle Memorial Institute, the Hudson Institute and the Stanford Research Institute. These and some 300 other think tanks think for a fee. Firms such as Arthur D. Little, System Development Corporation and Operations Research, Inc., have thought up such diverse items as the aerosol can, bus schedules to Reno's Harrah's gambling club and counter-insurgency tactics. The Stanford Research Institute has accepted jobs from both Israelis and Arabs. As its president commented, "We're not crusaders."

Finally there is a small handful of think tanks devoted to the study of a particular problem or kind of problem. Usually governed by a point of view—a feature noticeably absent from the other think tanks—these groups are reliant on the public, foundations, direct mail solicitations and other sympathetic individuals and groups. Some of the better known examples of these hard-working and poorly paid organizations are the Brookings Institution, the Center for the Study of Democratic Institutions and Ralph Nader's Center for the Study of Responsive Law.

Still Unknown Problems

Dickson includes a lengthy discussion of each category of think tank and provides plentiful examples of each. Throughout the book are assessments of the studies he mentions and of the think tanks producing them.

Think Tanks is an admirably objective book; it is the product of a journalist writing under a grant from the American Political Science Association. But one is constantly aware of a slight bias against think tanks as he reads along. Dickson tends to emphasize their danger to modern life rather than their positive contributions. It is true that several think tanks have created modern hazards by toying with weapons, war games and murderous chemicals. But it is equally true that in these uniquely twentieth century scientific cloisters will be found solutions for still unknown problems.

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Communications

(Continued from page 4)

lumping together missile-armed and gun-carrying ships is misleading. We had no intention of making a detailed comparison of the ships of the two fleets. In Table 2 we do distinguish Soviet ships according to armament carried, and in the case of submarines break this down by number of missiles per ship.

We are accused of resorting to the "numbers game" ourselves with respect to our comparison of Soviet and U.S. cruisers and frigates. Our introductory sentence to that section reads, "This question of cruisers and frigates provides an *apt example* of how cavalierly one can play the 'numbers game' in comparison of great-power armament capabilities." What follows, and what Polmar calls our numbers game, is merely the promised example. Concerning the Kashin class vessels that Polmar could not find among the frigates listed in our table; they are listed in the destroyer section. They are there because Raymond V. B. Blackman (*The World's Warships*, 1969, p. 107) calls them missile destroyers, and because *Jane's Fighting Ships* (1972-1973, p. 620) also calls them destroyers, not frigates.

We are criticized for our "ludicrous" comparison of U.S. and Soviet small craft. In the section of our article referred to, we state: "A numerical comparison between the United States and the Soviet Union with respect to these boats is *meaningless*. . . ." The comparisons Polmar mentions in his letter are not in our article; they are his own. The only numerical comparison in this section of our article is taken from a pro-Navy article in *Time* magazine, precisely to illustrate how "ludicrous" a comparison it is. The point we make in this section is quite simple and straightforward. The lack of a large fleet of missile patrol boats in the U.S. Navy is a result of U.S. strategy, not technological weakness.

Objections are made to our statement that NATO has never had or needed a cruise missile capability. The Harpoon, Penguin and Exocet are cited as examples of "frantic" NATO efforts to develop such weapons. A little naval history is relevant here. In 1955 the United States deployed the Regulus I, a jet powered cruise missile. It was not until 1958 that the first Soviet cruise missile, the SS-N-1, was deployed. In 1960 the United States had ready for production the Regulus II, a turbojet powered cruise missile, considered by the Navy to be superior even to the Shaddock cruise missile in current use on Soviet ships. In planning was an even more powerful cruise missile, the Triton, considered superior to any Soviet cruise missile present or planned (*Aviation Week and Space Technology*, Feb. 21, 1972, p. 66). All of these projects were abandoned in 1961. The U.S. Navy established the policy that the primary strike capability of the U.S. Navy, and hence of NATO's fleets, was to be invested in attack carriers, not cruise missiles. This policy has not changed, and the deployment of the comparatively short-range missiles referred to by Polmar certainly does not change it.

With respect to the use of Soviet cruise missiles against U.S. merchant ships, our contention was and is that in a planned attack on U.S. merchant ships the cruise missile submarines would stand idle. To successfully destroy U.S. shipping, the Soviets must have the ability to attack these vessels while simultaneously deterring or neutralizing any counteraction by U.S. carriers. Since cruise missile submarines are the most effective deep water weapon in the latter role, it is logical that they will be so used. To state merely that our view is "outdated" is hardly a substitute for a convincing alternative analysis.

Polmar criticizes our presentation of the relative building rates of NATO and Warsaw Pact countries. While he does not dispute our data, he implies that the last 3 years of construction is the relevant time period. Perhaps. We chose to quote the 5 and 10 years summaries contained in *The Military Balance 1971-72* (Institute for Strategic Studies). On the whole subject of construction, age and armament, Polmar and the ISS also differ. The ISS in its most recent analysis states: "as far as surface naval vessels are concerned it [the table] also shows that the NATO countries have generally been out building the Warsaw Pact, quantitatively and often qualitatively" (*The Military Balance 1972-1973, p. 92).*

We disagree with Polmar on how well-informed the public is on matters of budgetary commitments to new weapons systems. Except for the excellent analyses being made by a few Senators and by groups such as the Center for Defense Information in Washington, D.C., there is little effort being made, we feel, to educate the public on these issues. Few U.S. citizens have ever seen, much less waded through, the informative volumes of Congressional hearings on Department of Defense appropriations. Government and military officials, since they are the source of much data, have the especially heavy responsibility of presenting information to the public. When they shirk this responsibility by resorting to lies, half-truths and secrecy classifications they are preventing rather than encouraging a balanced discussion of the issues.

Finally, we are asked, "What is wrong with the Navy receiving the largest share of a budget?" A more important question, we feel, is "What justification does the Navy give for this large slice of the budget?" We insist that approval for naval modernization be based on more than vague phrases such as "maintaining our superiority at sea," or on simple numbers game-type arguments. The ULMS system, about which Polmar writes at length, is an excellent example. In our paper we presented a few of the "public" rationales advanced to justify its deployment. We then quoted Melvin R. Laird (then Secretary of Defense) as directly contradicting the key public reasons given for ULMS deployment. Our conclusion was that if these reasons were the *only* ones advanced for ULMS deployment, then that deployment was not justified.

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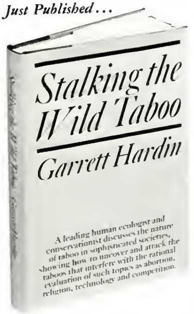

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